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MIND

A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY

EDITED BY
PROF. GILBERT RYLE
WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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MIND

A QUARTERLY REVIEW OF PSYCHOLOGY AND PHILOSOPHY

I.—INCREASE OF SUBSCRIPTION

At the Annual General Meeting of Mind Association on 13 July 1957 it was resolved to increase the payments for *Mind* as from 1 January 1958 to the following amounts:

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II.—TOWARDS A SCIENCE OF MIND

By James G. Taylor

The first attempts at transforming psychology into a scientific discipline were made not by psychologists but by men who had received their primary training in other scientific fields. Weber was an anatomist, Fechner a physicist, Johannes Müller and Helmholtz were physiologists, and Lotze and Wundt were medical men as well as philosophers. With this background it might have been expected that they would at least have laid a foundation for a scientific explanation of mental events; but in fact all that they achieved was a classification of sensory phenomena and of the associated stimulus energies. They could and did investigate the immediate physiological effects of such stimulus energies, but they were unable to formulate any theory as to how those effects could give rise to a set of events of an entirely different order, such as sensory and perceptual experience. The reason for this failure is not hard to find. Despite their scientific training the pioneers of experimental psychology were dominated by the Cartesian doctrine of psycho-physical dualism, and this determined the general form of the questions they sought to answer through their experiments. Those questions were not scientific questions in the ordinary sense, since they implied a postulate, not always explicitly stated, that was held to be so firmly established as to require no confirmation. This was the postulate that mind is an entity having none of the properties of matter, requiring, it is true, a physiological substratum for its expression, but operating according to laws that are completely independent of those of physiology.

Until this assumption was seriously challenged it was impossible for experimental psychologists to conceive of the qualities of colour, tone, taste, smell and the like, to say nothing of more complex phenomena such as perception, imagination, emotion, aesthetic experience and abstract thinking, as being properties of the same universe that provides the material for other scientific disciplines, and therefore impossible for them to begin constructing a science of mind. For, since Ryle[1] has clearly demonstrated that certain impossible things must be true if there exists a single instance of conscious experience being determined

[1] G. Ryle, *The Concept of Mind*, London: Hutchinson, 1949.

by the operations of a Cartesian " mind ", it follows that a discipline whose basic postulate is Cartesian dualism can never achieve the status of a science. If the foundation is non-existent the conceptual super-structure can have neither explanatory nor predictive value, and therefore is incapable of leading to the development of techniques for controlling and changing the facts.[1]

Clearly the first step towards the construction of a science of mind had to be the deliberate rejection of the concept of an independent mind-stuff, and this step was taken, more than forty years ago, by a small group of psychologists who called themselves Behaviourists. The significance of this event was widely misunderstood at the time, and there are still people for whom the term behaviourism is synonymous with the denial of the reality of conscious experience. But the real intention of Watson and his followers must be judged by their actions and not by their crude and belligerent statements about mind. By this criterion their purpose was to explain behaviour in mechanistic terms, that is, without invoking the intervention of an immaterial mind-stuff or " ghost in the machine ". They made no attempt to explain mental phenomena, although in the 1920's some of them foresaw that the new science they were developing would ultimately lead to the solution of that problem as well. Lashley [2], Hunter [3], Tolman [4] and others published articles in which they expressed their belief that this was an attainable goal and proposed tentative theories of functional relations between mind and behaviour. In general, however, behaviourists did not pursue this line of thought, because there was still a vast amount of investigation to be done before the scientific laws of behaviour could be stated with any degree of confidence. To this task they devoted themselves with so much energy that the study of behaviour is now widely recognised, even by its former opponents, as a legitimate science. The time has therefore come when it is possible to test the faith of some of the early behaviourists that their discipline would ultimately provide a solid foundation upon which a science of mind could be built.

[1] Freud elaborated his techniques despite the immaturity of the psychology of his time, but they are crude and wasteful just because they were not based on a solid scientific foundation.

[2] K. S. Lashley, 'The behavioristic interpretation of consciousness', *Psychol. Rev.*, 30 (1923), 237-272, 329-353.

[3] W. S. Hunter, 'The problem of consciousness', *Psychol. Rev.*, 31 (1924), 1-31.

[4] E. C. Tolman, 'A behaviorist's definition of consciousness', *Psychol. Rev.*, 34 (1927), 433-439.

I

I propose to base my argument on the principles of behaviour elaborated by Hull [1]. It is true that those principles are not yet fully accepted even by professed behaviourists, but they do at least conform to the requirements of a coherent scientific theory, in that they comprise a series of postulates each of which is in agreement with a substantial body of experimental evidence, and a much larger number of theorems and corollaries, derived from the postulates and also in agreement with experimental evidence.

It will be useful to consider briefly the nature of this evidence. It is derived from experiments in which certain uniformities are introduced into a situation to which the experimental subjects are repeatedly exposed. Some of those uniformities are of an arbitrary character and depend upon the experimenter acting consistently according to the plan of his experiment. Thus the presence of food in the goal-box of a maze is determined not by the natural properties of the maze but by the design of the experiment. Or, to take an example from an early experiment by Thorndike, the opening of the door of a cage containing a hungry cat as soon as the cat licked itself was determined by the temporary intention of the experimenter and not by any natural and necessary properties of the experimental situation. There are in every experiment uniformities that are not arbitrary but spring from the natural properties of the materials used. Thus if the experimental animals are placed in a maze twenty-three hours after their last meal there are certain physiological processes that are directly determined by this fact and are not subject to control by the experimenter, except in so far as he arbitrarily decides on this period of privation. Again, the experimenter cannot control the odorous gases given off by the food he places in the goal-box, although he can, if he chooses, prevent them from reaching the noses of his subjects or from inducing afferent impulses in their olfactory nerves. Or, to take another example, the wooden walls of the maze have certain invariant properties which ensure that the animal's movements will be confined within a space chosen by the experimenter. If he chose to construct a maze out of materials with very different properties, such as tissue paper or gingerbread, the character of the experiment would be completely changed.

The results of innumerable experiments show that whether these uniformities be of natural or arbitrary origin they invariably

[1] C. L. Hull, *Principles of Behavior*, New York : Appleton-Century, 1943; '*A Behavior System*', New Haven : Yale University Press, 1952.

determine, sooner or later, certain ultimate uniformities in the behaviour of the experimental subjects. Thus if the walls of a maze are constructed of some resistant and inedible substance, rather than paper or gingerbread, the animals soon cease to press against the walls, to explore cracks and other irregularities, to climb to the wire mesh that forms the roof of the maze, and learn to run along the alleys without more than casual contact with the walls. If food is always placed in the goal-box and never at the ends of any of the blind alleys the path followed by the animal will ultimately become standardised, and if the food privation is held constant a standard rate of running along this standard path will be established. Further, the rate at which these responses become standardised is a joint function of the duration of food privation, the quantity of food the animal is allowed to consume in the goal-box, the amount of work it has to do to reach the goal and the time that elapses between the beginning of the run and the beginning of the consummatory reaction of eating.

These results are summed up by Hull in what he calls the principle of reinforcement. Stated briefly and simply, this means that whenever a compound pattern of stimulation, including exteroceptive elements determined by an external situation and interoceptive elements determined by some physiological need such as hunger, evokes a specific response, and that response is followed by a reduction in the strength of the interoceptive elements of the stimulus compound, there is an increase in the probability that when the same stimulus compound is presented again this response will be the first one to be evoked. It is obvious that a situation characterised by certain uniformities, whether artificially contrived or not, must, according to the principle of reinforcement, result in repeated increments in the probability that the successful response will be the first to appear, so that behaviour in this situation must ultimately be standardised.

Complementary to the principle of reinforcement is the principle of extinction, which states that the probability that some specific response will be evoked is diminished whenever that response is followed by either no change in the interoceptive stimuli or an increase in their intensity or the addition of further stimuli such as may be produced by injury to the tissues. If we use the term " negative reinforcement " instead of " extinction ", then both positive and negative increments in the probability that a given response will be evoked can be comprised under the single principle of reinforcement.

An important extension of the principle of reinforcement is

that whenever an animal has learned to respond in a standard way to a compound pattern of stimulation including exteroceptive elements due to some external object, A, these stimuli acquire the power of reinforcing a response to another object, B, which stands in some invariant relation to A, provided that the response to B has the effect of bringing the animal into contact with A. This process is known as secondary reinforcement, and its effect is that in any situation in which various objects stand in fixed relationships to one another, an encounter with any one of them initiates a train of responses culminating in one which is likely to satisfy the animal's primary need.

Hull studiously avoided speculations concerning the physiological basis of the habits acquired through the process of reinforcement, but since an organism that has learned a standard response to a specific situation can still respond in that way after a long interval during which the situation has not been encountered, it is clear that the immediate cause of the response must include some relatively permanent feature of the organism itself. The earlier reinforcement has induced some change, whether structural or chemical or both we do not know, in the nervous system, such that the afferent energy generated by the impact of the situation on the sense organs tends to be directed to the motor channels and to evoke the reinforced response. We may use the word *engram* to refer to any organisational feature of the nervous system induced by reinforcement. It should be noted that negative reinforcement produces its engrams as well as positive reinforcement, and there is plenty of evidence to show that when an existing habit is extinguished by negative reinforcement the old engram is not destroyed but is simply prevented by the new engram from taking control of the effector system.

There are various other principles in Hull's system, but as they are not so directly relevant to the problem of giving a scientific explanation of mind they will not be enumerated here.

II

The principles enunciated by Hull were derived from carefully planned laboratory experiments in which certain uniformities, often of an arbitrary and artificial character, were introduced, and numerous factors which in everyday life situations are subject to variation, continuous or stepwise, were held constant. But this does not mean that the principles of learning that apply in the laboratory are not equally applicable in everyday life. Although there are few planned uniformities, and these are seldom applied

with the rigour that is customary in a laboratory experiment, there are plenty of uniformities that are dependent on the natural properties of the various objects and substances in the environment. For example, the direction of the gravitational pull of the earth is constant, and every organism must therefore learn, through the negative reinforcement induced by loss of support, to walk and run without falling, and when carrying or manipulating objects to keep its centre of gravity within the area of safety.

Consider another example. An animal on which another animal preys has a characteristic size and shape and colouration, moves in a distinctive manner, makes certain sounds as it moves, utters certain cries, gives off a distinctive odour and leaves traces of this odour on the ground it has traversed, searches for food at certain times and in certain localities and shelters from its enemies in other types of locality. These properties can stimulate the sense organs of the predator, and if it responds to them by a train of actions which culminates in the seizure of the prey it thereby begins to acquire a standard pattern of behaviour through which its survival is ensured. Sometimes it will fail, and the result is the negative reinforcement of certain components of its behaviour, such as movements which reveal its presence prematurely to the intended victim. There are plenty of uniformities here, as determined by the natural properties of the prey, but there is a great deal of variation as well. The victims differ from each other in age and experience, in speed of response and in cunning; and the configuration of the geographical environment in which the attack occurs is likely to be different on every occasion. But that presents no difficulty, for in the course of its playful activity while it was still under the protection of its mother it acquired responses to a large variety of features of its environment, learning to leap over or run round obstacles or to hide behind them, to modify its running posture according as it was going uphill, downhill or on the level, and the like. And subsequently it learned to respond appropriately to every permanent feature of its normal hunting ground.

In short, the natural environment in which any animal grows up is full of uniformities of the kind which the behavioural scientist deliberately introduces in his experiments, and in so far as they are relevant to the needs of the animal these uniformities necessarily give rise to standard responses to a wide variety of objects and situations in the environment. In fact, the most striking thing about the early life of any complex organism is the enormous number of habits it must acquire in the course of its interaction with its environment, and the enormous number of

engrams, mediating those habits, that have to be laid down in its nervous system.

Structural psychologists following in the Wundtian tradition used to believe without question that stimulation of the animal's sense organs automatically and immediately gave it a knowledge of certain properties of its environment by virtue of the perceptual "experience" which this stimulation was assumed to induce. And this belief carried with it the implication that the experiences created by sensory processes somehow mediated the initial learnings whereby the animal began to "know" its environment. Thus it was assumed that before an animal could learn to treat a tree or a rock as an obstacle to its forward progression it had to have some kind of pre-perceptual experience, or "pure sensation", aroused by the obstacle. The subsequent full perception of the obstacle as a tree or a rock was then thought of as being compounded of the primitive pure sensation and memories of other sensations aroused by closer contacts with the object.

This is Cartesian dualism naked and unashamed, and it is of course totally unacceptable to the behaviourist. His retort to this kind of statement is not merely that it errs in assuming knowledge of the sensory and perceptual experience of a young animal, which we do not possess even if the animal happens to be human, but that it violates the principle of parsimony. We know that stimulation of the sense organs by any external object gives rise to impulses in the afferent nerves, that these trigger responses in the motor nerves and in the muscles, that if the resulting action alters the physiological condition of the organism the pattern of stimulation associated with that condition is altered, and finally that this modifies the connexions between afferent and efferent nerves in such a manner that the probability of the same response being again evoked by the external object is increased or decreased according to the nature of the physiological changes that followed this response. In short, behaviour can be explained without postulating any kind of mental experience as a link in the causal chain between stimulation and response.

III

To deny causal functions to mental experiences is not equivalent to denying the reality of such experience. It means rather that we frame our questions about it in a different way. We no longer ask, as many psychologists of the nineteenth century did, what is the relation between mind and body, with the implicit understanding that the answer must be some form either of

parallelism or of interactionism. Nor do we ask, as some modern psychologists do, to what extent our mental experience is modifiable by learning, for that question implies the belief that there is an original core of conscious experience which is ultimately unexplainable, just as electrons, protons and other particles are at present assumed to be the ultimate constituents of matter. It seems to me that the only kind of questions about mind which can qualify to be called scientific are those which start from the assumption that every form of conscious experience without exception is a function, or more likely a complex system of functions, of the living organism.

I assume that such functions are definable with the same degree of precision that prevails in the exact sciences, that a science of mind is born as soon as this criterion is satisfied by a single function, and that the growth of such a science is to be measured by the increase in the number of functions that can be so defined.

The early psychophysicists assumed that a substantial number of functions with the required degree of precision could be written. In these functions the dependent variable was always some mode of sensory experience and the independent variable was some stimulus energy applied to a sense organ. But is soon became apparent that the supposed precision was illusory. It was observed, for example, that a sheet of white paper continues to be perceived as white when it is seen in dim light and the energy of the light it reflects into the eye is substantially less than that of the light reflected by a black surface in stronger illumination. The phenomena of hysterical anaesthesia show that a stimulus may produce no sensory or perceptual experience of any kind in a person who is awake and alert and whose sense organs and nervous system are intact. More recently the data collected by Senden [1] show that the visual experience of a patient who has just recovered from an operation for congenital cataract lacks many of the properties of normal perception but acquires those properties in the course of time. Finally, experiments involving the prolonged wearing of spectacles that distort the visual field in some way show that perceptual and sensory experience is by no means a fixed function of the stimulus energies applied to the sense organs even under standard conditions of testing. Kohler [2] reports an experiment in which he wore spectacles which caused the right half of the visual field to be tinted blue and the left half

[1] M. v. Senden, *Raum- und Gestaltauffassung bei operierten Blindgebor enen vor und nach der Operation*, Leipzig : Barth (1932).

[2] I. Kohler, *Über Aufbau und Wandlungen der Wahrnehmungswelt*, Vienna : Österreichische Akademie der Wissenschaften (1951).

yellow, the dividing line between the two being in the median plane of the head. As the experiment went on from day to day the distinction between the two colours faded progressively, and when the spectacles were removed the right half of the field was seen to be tinted yellow and the left half blue. In another experiment spectacles in the form of wedge prisms were worn. These produce bands of colour at all vertical edges where there is a sharp change of illumination. Thus a white rectangle is bounded at one side by bands of yellow and red and at the other side by blue and violet. These colours fade and ultimately disappear, and when the spectacles are removed yellow bands are seen at edges which previously were blue and violet, and blue bands where previously yellow and red were seen. What is even more striking is that those same colours are seen when a white rectangle is viewed by monochromatic sodium light. One of the edges is seen as blue although there are no " blue " rays in sodium light.

These facts point to the conclusion that the independent variables investigated by the early psychophysicists are not those to which sensory experiences are functionally related. There is, of course, no sensation without some form of stimulation, and this suggests the hypothesis that sensation is a function not of the stimulus energy itself but of some other function of that energy. Thus, instead of writing, as Fechner did, $S = k \log R$, or simply $S = f(R)$, we shall write

$$S = F(\phi(R))$$

where $\phi(R)$ is some function of stimulation which has not yet been considered by psychophysics.

It is here that the work of the behaviourists becomes relevant to the psychological problem, for they have demonstrated a number of functional relationships in which stimulus energies are among the independent variables. However, the functions in question are not simple functions of stimulation, and this point requires a little elaboration before we can proceed. The only functions in which stimulus energies are the sole independent variables are those that are concerned with the transmission of neural energy to the cortical projection areas; and apart from reflex actions there are no modes of behaviour which are simple functions of stimulation. That is, there is no form of non-reflex behaviour which is necessarily linked, by virtue of the structure of the nervous system, to any specific stimulus. The plasticity of the nervous system is such that, in principle, any stimulus may be made to evoke any response we choose, although

in practice the uniformities that abound in nature ensure that much of the behaviour acquired by an individual is identical in its main characteristics with that of all normal members of the species.

An important consequence of the plasticity of the nervous system is that various measurable characteristics of behaviour must be described not as direct functions of stimulation but as functions of certain engrams that have been built into the brain through the process of learning and have the property that when aroused to activity by the appropriate stimulus they tend to evoke the associated response. Hull uses the symbol ${}_sH_R$ for an engram which mediates a specific response R to a specific stimulus S, and there are two kinds of functions in which it is involved, *viz.* one type in which it is the independent variable and another in which it is dependent. As examples of the former we have the statements that the probability that R will be the first response to a given presentation of S is an increasing function of ${}_sH_R$, and that the time between the presentation of S and the beginning of R is a decreasing function of ${}_sH_R$. ${}_sH_R$ appears as a dependent variable in the statements that ${}_sH_R$ is an increasing function of the number of times the temporal conjunction of S and R has been followed by reinforcement, and a decreasing function of the interval of time between the beginning of R and the beginning of the reinforcement. There are other functional relations of both types but they will not be enumerated here.

We are now able to give a more precise meaning to our hypothesis that sensation is a function of a function of stimulation; but in order to avoid confusion between Fechner's notation and Hull's we shall use the symbol Π (for perception) in place of Fechner's S (sensation), and S for stimulus instead of Fechner's R (*Reiz*). Our hypothesis then reads

$$\Pi = \mathrm{F}(\phi(S))$$

and the immediate question that now confronts us concerns the form of $\phi(S)$. The only simple functions of S are those that are concerned with the transmission of afferent impulses to the cortex, but this was precisely the kind of function that the early experimental psychologists postulated as the basis of sensory experience and, as we have seen, it is quite incapable of carrying the burden. We are then left with the acquired functions of S that have been revealed by behavioural science. Our hypothesis then takes the more precise form

$$\Pi = \mathrm{F}({}_sH_R)$$

which implies that perception is a function of the habits which the organism has acquired in the course of its life.

We have next to consider the form of the function F, and in this we must obviously be guided by the known properties of perceptual experience. Three of those properties will be considered here. The first is that perception can occur in the absence of overt responses. That is, although is it necessary that S be presented it is not necessary that R be evoked. Secondly, perception is normally a complex experience in which a large number of elements are present simultaneously. We are simultaneously aware of the various objects in our immediate environment, of their positions relative to one another and to ourselves, their sizes, shapes, colours, names, uses, ownership and a host of other properties too numerous to mention. And lastly, this complex experience has the very important property of constancy. That is, the environment as we perceive it remains in the same place, retains all its internal relations and its shapes, sizes, colours and the like, despite continuous variation in the pattern of stimulation to which it gives rise.

The first of these properties enables us to specify that $\mathrm{F}({}_{S}H_{R})$ is simply the arousal of activity in the engram by the presentation of S. Whether R is evoked or not is immaterial as far as perception is concerned. The second property enables us to state that the function F applies not to a single engram but to a set of engrams, which is necessarily always a subset of the set of all the engrams existing in the brain of a given organism. We may use the symbol **S** to represent the set of stimuli emanating from the actually present environment, and **R** to represent the set of learned responses to the elements of **S**. It is to be understood that there is a one-to-one correspondence between **S** and **R**, such that for every $S_{\mathrm{i}} \in \mathbf{S}$ there is an $R_{\mathrm{i}} \in \mathbf{R}$ which has been linked with it through a process of learning, and for every $R_{\mathrm{j}} \in \mathbf{R}$ there is an $S_{\mathrm{j}} \in \mathbf{S}$ to which it has been similarly linked. The symbol ${}_{\mathbf{S}}H_{\mathbf{R}}$ will then represent the set of engrams mediating all the acquired responses to the stimuli that are present in the environment and acting on the sense organs. It should be noted that stimuli that are present and acting on a sense organ but to which no learned responses have been linked are not included in **S**. Our hypothesis may now be stated in the form

$$\Pi = \mathrm{F}({}_{\mathbf{S}}H_{\mathbf{R}}).$$

The definition of the elements of **S** has been deliberately left vague pending discussion of the significance of perceptual constancy, which we must now take up. Various examples of

constancy have been described and discussed by psychologists and philosophers, and the selection of those examples seems to imply that constancy is a feature of some kinds of perception but not of others. It is in reality a universal property of perception, and there can be no perception without it. The essence of it is that the states of stimulation in the sense organs resulting from the action of the stimulus energies coming from the external environment are subject to continuous variation with changes in the position and orientation of the organism relative to the environment, the position of the head relative to the body and the direction in which the eyes are facing relative to the head; but throughout all this change of stimulation there is no change in the perceived properties of the external field. That is, a large number of different states of stimulation can all give rise to the same perception.

Now according to our hypothesis the function of stimulation of which perception is a function is one that has been established by a process of learning; and the facts of perceptual constancy therefore require that the functions of the several states of stimulation that can be aroused by an external object must all have identical values. That is, if the relevant states of stimulation are symbolised by S_a, S_b, . . . S_n, the hypothesis requires that $\phi(S_a) = \phi(S_b) = \ldots = \phi(S_n)$; and this condition is clearly satisfied if S_a, S_b, . . . S_n are all linked by the learning process to one and the same response R.

In general, this condition cannot be satisfied if the relevant states of stimulation are determined solely by the stimulus energies from the external field, since there are many situations in which an S_i from one object, with the body and sense organs in one position, is identical with an S_j from another object when the body and sense organs are differently placed. Thus the same set of retinal receptors may be stimulated by a vertical line when the head is erect, by a horizontal line when the head is reclining on a pillow, and by lines in numerous other directions when the head is, in each case, tilted through the appropriate angle. However, each of these different positions of the head determines a specific pattern of proprioceptive stimulation, and it is easy to see that a line in a given objective direction, and a given orientation of the head together determine a compound pattern of retinal and proprioceptive stimulation which is unique and can therefore be unambiguously conditioned to some specific response such as tracing the line with a finger. If the orientation of the head is changed without change in the direction of the line, both retinal and proprioceptive stimulation are altered, but the

compound stimulus pattern is again unique and can be conditioned to the same response of tracing the line.

This discussion leads to two important generalisations, which incidentally will serve to define **S**. The first is that in general each $S \epsilon \mathbf{S}$ is a compound pattern of stimulation involving two or more receptors. One of the components of this compound is directly determined by stimulus energies from the external object, and as a rule another component will be proprioceptive stimulation determined by the posture of the body. There are some compounds that include interoceptive stimuli and others in which the components are all exteroceptive. No learned reaction is sufficiently determined by a single stimulus, and this implies that the engrams mediating such behaviour are complex structures involving neurons in several different parts of the brain.[1]

The second generalisation is that each $S \epsilon \mathbf{S}$ is at the same time an element of what we may call an *equivalence class* of stimulus compounds, which we shall designate E. The elements of any E are equivalent to one another by virtue of the fact that each of them has been conditioned to the same response or set of responses, R. To say that S_a, S_b, . . . are all elements of a single E is just another way of saying that $\phi(S_a) = \phi(S_b) =$. It is clear that each orderly aspect of nature that is capable of affecting the sense organs and at the same time is directly or indirectly relevant to the needs of an organism must determine an equivalence class of stimulus compounds.

We can now define **S** as a set of stimulus patterns with elements $S_1, S_2, \ldots$, such that each S is a unique compound stimulus that has been conditioned to a specific response, and such that each S is an element of an equivalence class, E, of compound stimuli which have all been conditioned to the same response. It will be seen that any change in the position or posture of the organism or in the orientation of its sense organs will change some or all of the elements of **S**. But each of the new elements is drawn from the same E as its predecessor, so that there is no change in the one-to-one correspondence between **S** and **R** and no change in the composition of **R**. Some elements of **S** may be eliminated and new elements added, and corresponding elements will be eliminated from and added to **R**. If we neglect the added and eliminated elements, and define two new sets, $\mathbf{S}'$ and $\mathbf{S}''$, by the condition that for each $S' \epsilon \mathbf{S}'$ there is an $S'' \epsilon \mathbf{S}''$ and for each $S'' \epsilon \mathbf{S}''$ there is an $S' \epsilon \mathbf{S}'$, then we can say that $\mathbf{S}'$ is equivalent to $\mathbf{S}''$ in the same defined above. It then follows that

[1] This explains why attempts at localising specific mental functions in the brain have hitherto been unsuccessful.

$$\Pi = \mathrm{F}({}_{\mathbf{S}}H_{\mathbf{R}}) = \mathrm{F}({}_{\mathbf{S}}H_{\mathbf{R}})$$

expresses a determinate relationship such that Π is known in detail as soon as we know all the elements ${}_{S}H_{R}$ that are subsumed under F, and the equivalence class, E, to which each one belongs.

Finally, it must be observed that whenever any subset of **S** is conditioned as a whole to a specific response, this determines another element of ${}_{\mathbf{S}}H_{\mathbf{R}}$. These subsets may be of any degree of comprehensiveness, to the extent that the whole of **S** may be a subset of itself provided it is conditioned to a response such as naming the locality one is in. For example, if I stand in front of Botticelli's *Birth of Venus*, a particular patch of paint activates an engram that enables me to perceive the exact position of that patch; a group of such patches arouses an engram which mediates my perception of a part of the picture as representing an arm; a still larger group enables me to perceive this figure as Venus; and a yet more comprehensive set of stimuli, nearly the whole of **S**, mediates my knowledge that I am in the Uffizi Gallery in the city of Florence. We can now complete our definition of **S** by saying that its simplest elements, besides being elements in their own right, may be combined in a wide variety of ways to form subsets which are themselves elements of **S**.

Experimental evidence in support of this hypothesis is presented elsewhere [1] and will not be repeated here.

IV

The theory outlined above has a number of important theoretical and practical implications, some of which are discussed in the following paragraphs.

1. If every mental experience is a function of a set of engrams built into the nervous system by a process of learning, it follows at once that there is no mental experience at birth—not even "pure sensation". The infant's mind is correctly described as a *tabula rasa* (although *tabula vacua* would be more accurate, since nothing has previously been inscribed on it), because learning cannot begin as long as the infant's physiological needs are continuously and adequately supplied through the umbilical cord. It follows too that we must reject the Freudian hypothesis that certain characteristics of neurotic experience are attributable to "unconscious memory" of the birth trauma and an "unconscious desire" to return to the safety and comfort of the womb.

[1] J. G. Taylor, *The Behavioural Basis of Perception*, in perparation. J. G. Taylor, and S. Papert, 'A theory of perceptual constancy', *Brit. J. Psychol.*, 47 (1956), 216-224.

The infant cannot be born again and again, and there is therefore no opportunity for acquiring differential responses to the intrauterine and external environments, which alone could serve as a basis for such memories and desires.

2. It is now possible to give a clear answer to the hoary question whether infrahuman animals have minds, and to the associated question whether anyone else besides oneself has a mind. The answer is that every organism that is equipped with a plastic nervous system enabling it to adapt itself to the ever changing features of its environment acquires mind to the extent that its adaptation results in the acquisition of stable habits. A rat that has learned to respond to visual stimuli by avoiding damaging contacts with obstacles in its way, to follow paths that lead to food, to react to certain auditory stimuli by taking cover and the like, acquires mental experience that is limited by the humble range of its achievements but is nevertheless unmistakably mental. Similarly the old question whether we can know for certain whether other people have minds is answered in the affirmative. To be sure we cannot know the detailed contents of another person's mind, but the reason for this is the strictly practical one that we cannot know the entire life history of anyone else and therefore cannot know all the habits he has acquired or how they are organised.

3. We can deduce from our knowledge of the properties of the material environment that are capable of acting directly on our sense organs, and of the relevance of those properties to the biological needs of the organism, that there are certain elements of mind that must be shared in common by all people with a normal equipment of sense organs and nervous system. That is, there are certain modes of behaviour that every individual *must* acquire if he is to survive. Thus we must all learn to react appropriately to the spatial order of the objects in our environment, and out of that learning emerges a perception of space which must be essentially the same in all of us. Again, the conditions under which the eyes and ears can be stimulated by light and sound respectively are such that we must all acquire differential responses to visual and auditory stimulation, and therefore must establish in our minds a clear distinction between visual and auditory experience.

On the other hand there are many refinements of behaviour that are imposed upon us not by biological compulsion but by social pressure, and we can therefore deduce that where those refinements exist there are elements of mind that are absent in others. Thus there is no biological need to react differentially

to tones of 256 and 257 cycles per second, and we can expect that there will be no perceptual difference between them unless differential behaviour is demanded by social convention, as when one is learning to sing or to play the violin. Tone-deafness in the absence of physiological defect is fairly common in Britain and America but is virtually unknown among certain African peoples whose language makes extensive use of intonation of the voiced sounds. To take a personal example, my own perception of colour distinctions is primitive as judged by modern standards, although I am not colour-blind, because I have not the habits of the painter who can in a moment select the proportions of pigments that will give him the tones he requires, nor do I buy lipsticks to harmonise with the colour scheme of my clothing.

4. The distinctions between the broad categories of mental experience are directly deducible from an analysis of the elements of ${}_{\mathbf{S}}H_{\mathbf{R}}$. Consider, for example, the sensory modalities. It is a commonplace that visual experience is mediated by elements of **S** whose exteroceptive components are retinal stimuli, and formerly it was assumed that this alone was sufficient to determine the quality of the experience. Modern neurophysiology affords no support to this view. However, if we look at the corresponding elements of **R** we find that they too are characterised by certain properties which they all share in common and which are not to be found in the responses to stimuli applied to other sense organs. The most obvious of these common properties is that the reactions in question, which are initiated by stimuli from objects that are mostly not in contact with the organism, are accurately adjusted to the distances and directions of those objects. There is no other type of stimulation to which we can learn to respond in this precise manner, although there is some approximation to it in blind people who have learned to respond to the echoes of their footsteps and voices, thrown back to them by the objects in their vicinity, by moving freely among those objects. They find their way about in much the same way as the bat does, but the bat is more efficient because the short wavelength of the sounds it utters enables them to be reflected by much smaller objects. The blind man, even if he has developed this " sixth sense " to the maximum degree, must complete his movements towards an object by groping with his hands, whereas visual stimulation enables us to complete the action in a single direct move.

But if the responses conditioned to visual stimuli fall into a distinctive class by virtue of their possessing certain properties in common ; and if the responses to auditory stimuli fall into another

class by reason of their having other common properties, different from those of the responses to visual stimuli; and similarly for responses to tactile, gustatory and olfactory stimuli, then it follows from our hypothesis that each of those classes of stimuli constitutes a super-ordinate equivalence class and therefore determines a perceptual constancy. That is, despite wide differences in detail, the mental experiences aroused by a variety of retinal stimuli must all have a common quality that is not to be found in the experiences aroused by other modes of stimulation. In short, the theory enables us to account for the sensory modalities without having to postulate unobservable and unverifiable specific energies either in the afferent nerves or in the cortical projection areas, or in any other part of the brain.

The most convincing evidence in support of this explanation is that the blind man perceives the echoes of his footsteps and voice not as auditory phenomena but as minute pressures exerted by the objects in his neighbourhood against the skin of his face. He has acquired a class of responses to auditory stimuli which differs in its properties from the class of responses that in normal people are evoked by stimulation of the ear, and in doing so he has created a new sensory modality. It is not for nothing that common parlance attributes to him a sixth sense; only there is no sixth sense organ. For him the ear is the organ of two clearly differentiated senses.

By analogous reasoning we can explain the characteristics of other categories of mental experience such as imagination, memory, emotion, knowledge, conceptual thinking and the like.

5. It is now possible to offer a simple explanation of what William James called the stream of consciousness. But first we must consider what is meant by consciousness. It is clear that when any set, ${}_{S}H_{R}$, of engrams is operated upon by the function F, the organism is thereby put into a state of simultaneous readinesses to respond appropriately to any aspect of the environment that may prove to be relevant to its needs and purposes. This, I submit, is the essence of consciousness. It is a private experience, and it is inaccessible to public observation for a very simple reason. An internal state of preparedness cannot become apparent until the occasion for action arises. In the system we are considering a large number of engrams converge on a limited number of effector organs, each of which is capable of acting in only one way at a time; therefore only a limited number of elements in a multiple state of preparedness can become manifest in any moment of time. The simultaneity of the process as

such has no means of making itself known to another person, but it is none the less real to the person in whom it occurs.

It is clear that every change in a person's position and orientation involves a change in the selection of engrams that are operated on by the function F, and this implies a change in the state of multiple preparedness, that is, in conscious experience. But this does not constitute the entire stream of consciousness, except perhaps in the case of the retired worker who described his daily life by saying: " sometimes I sits and thinks, and sometimes I just sits." When he sits and thinks he is responding verbally and in other symbolic ways to various stimuli, and these responses themselves provide stimuli which serve to arouse activity in other groups of engrams, thereby bringing new elements into the stream of consciousness. Since the language habits of each person constitute a unique system, it follows that the streams of consciousness of two persons, even when placed in the same situation and facing the same urgent problem, can never be identical.

The stream of consciousness periodically shrinks to a trickle and stops flowing—we go to sleep. This phenomenon finds a ready explanation in terms of the science of behaviour. Interoceptive stimuli incidental to a condition of general fatigue become conditioned to the inhibition of all learned responses; which means that when those stimuli are present and not counteracted by a strong drive, no activity can take place in any engrams mediating positive behaviour. Since our theory is that mental experience is the simultaneous activity of a set of engrams, it follows that the inhibition of all engrams is characterised by a complete absence of consciousness. A person who changes his abode to a locality where the nocturnal noises are of a different character cannot sleep at first. He has not yet acquired conditioned inhibition of his responses to the unusual sounds.

The partial failures of consciousness that occur in cases of hysterical anaesthesia are due to the conditioned inhibition of all responses formerly evoked by stimulation of one sense organ, although the inhibitory stimuli are not necessarily associated with fatigue.

6. In the past the growth of mind has frequently been considered to be analogous to the development of a plant from the seed. The properties of mind were assumed to be potentially inherent in something analogous to the germ plasm and required only time and a favourable environment for their unfolding. Even today the advocates of " non-directive therapy " seem to hold this view. But our theory leads to the conclusion that the

growth of the mind is a joint function of the capacity of the nervous system for building engrams and the system of reinforcements provided by the environment; and since a large and very important part of this environment is man-made, it follows that the human mind is largely the product of human institutions and is not determined by some mysterious destiny from which there is no escape. And since human institutions are transient so also are certain man-made features of the human mind.

But, knowing this, we obviously cannot be content to leave such changes to the hazards of historical development, hoping vaguely that everything will turn out for the best in the best of all possible worlds. We want to be able to change our minds to our own advantage by deliberate application of scientific principles, and this in fact can already be done in the case of individual mental disorders. Phobias and other neurotic disturbances can be cured without residue at a speed that appears miraculous in comparison with older techniques such as psychoanalysis and its various derivatives. The essence of the procedure is to identify the elements of behaviour that are responsible for the mental distress and to eliminate or transform them by a carefully planned programme of re-education. As a rule the most recently developed symptoms are tackled first, since the relevant habits are less firmly entrenched. The patient himself is made responsible for carrying out the programme, and rapid success in dealing with the first symptom provides powerful reinforcement for the action of changing his own faulty behaviour. He thus becomes his own psychotherapist and often surprises the initial therapist by announcing that the major symptoms have paled into insignificance before he has received any directives concerning their treatment.

Such results also strongly reinforce the scientific behaviour of describing mental experience as a function of the innumerable habits the individual has acquired in the course of his life. The laws governing scientific belief are essentially the same as those governing perception and all other modes of mental experience.

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III.—COUNTERFACTUAL STATEMENTS AND LOGICAL MODALITY

By B. K. Milmed

Counterfactual statements may be identified as statements that "If p were true, then q would be true"; *i.e.* assertions whose antecedent and consequent are known or assumed to be false, and whose truth or falsity is determined independently of that fact. Modal statements are statements involving the concepts of necessity, possibility, and impossibility. It is obvious that these two classes of statements may overlap. On one hand, all valid statements asserting a logically necessary, *i.e.* tautological, relation of antecedent and consequent are true independently of the truth of the components, which therefore may sometimes be false; and on the other hand, all conditional statements whose truth-value is independent of the truth or falsity of their components must depend for their truth upon a connexion between the components that is in some sense necessary.

In order to separate the two types of statement to facilitate discussion, we may find it helpful to restrict the term "counterfactual" to a statement which has something to do with *facts*; more precisely, one that asserts a relation inferred from experience rather than from logic, and whose components are empirical statements factually false. The term "modal" will be applied to statements that assert *logical* necessity, possibility, or impossibility only; and a statement will be regarded as logically necessary when the truth of the consequent can be inferred from the truth of the antecedent without any further information except the axioms and rules of logic. For purposes of the present discussion, the only modal statements with which we need be concerned are those involving necessity.

It will easily be seen that the difficulties associated with the analysis of counterfactuals, too much discussed to require detailed restatement at this time, may be interpreted as revolving around the very distinction drawn here. For they concern primarily the apparent need for a connexion between antecedent and consequent that is both extra-logical and necessary, in the face of a common recognition that there can be no necessary

connexion except a logical one. It may be advisable, then, to explore the extent to which the concept of *logical* necessity, adequately interpreted, can serve to explain the truth of counterfactual statements, adequately interpreted. To do so is the purpose of the present discussion.

It will be apparent that the two types of statement, even as deliberately differentiated by the definitions just presented, possess resemblances warranting a joint approach to them. First, each represents a kind of relation expressed by "if . . . then" in ordinary language. It is true that an expression of ordinary language need not be accorded recognition by logicians, and that this one in particular is rendered suspect by the variety of meanings that it covers and by the somewhat different and more specific meaning assigned to it in its technical use. Yet, as is often pointed out, all uses of this expression, both in ordinary language and in logic, have the common feature that if the antecedent is true, the consequent is true also. Moreover, because of this feature, as is less often noted, all kinds of "if . . . then" relation provide a basis for genuine inference.[1] Even in its customary logical use in the form of material implication, the importance of such a relation for inference is shown by its role in the indispensable "rule of detachment". In other uses, especially its modal use, the "if . . . then" expression indicates relations even more readily available for inference.

Counterfactual and modal statements, moreover, differ from material implication along partly similar lines, since both represent narrower relations; *i.e.* the components of any counterfactual or modal statement are connected by the relation of material implication, but also by further relations from which other cases of material implication are excluded. Counterfactuals, as Goodman observes, if they were interpreted in terms of material implication alone, would all be true, since all of their antecedents are false; so that some additional qualification is needed to distinguish between true and false counterfactuals.[2] As to modalities, Lewis and Langford make a parallel observation. All true statements of modal necessity are true as material implications, but the converse does not hold; for in terms of material implication every true statement implies every other, but in terms of modal implication we cannot *deduce* every true statement from every other (p. 238). We see, then, that neither

[1] C. I. Lewis and C. H. Langford, *Symbolic Logic* (New York, The Century Company, 1932), p. 236.

[2] Nelson Goodman, *Fact, Fiction, and Forecast* (Cambridge, Harvard University Press, 1955), pp. 13-14.

counterfactual nor modal implications hold in any case where material implication does not, but that both differ from it in failing to hold in some cases where the antecedent is false. Modal implications differ further in failing to hold in some cases where the consequent is true, a situation which by definition never arises for counterfactuals.

Finally, and most important, both counterfactual and modal statements assert relations between antecedent and consequent which are to be regarded as obtaining independently of the truth of the components, and as necessary in that sense.

An attempt to analyse counterfactuals without reference to any special kind of connexion at all has been offered by Hiz. This attempt is of particular interest because it turns out, on analysis, to provide not only for a connexion but for an explicitly modal connexion; and it is through this implicit modality that Hiz supplies a very helpful mechanism for the further elucidation of counterfactuals. A counterfactual statement, according to his analysis, occurs "in a meta-language", and "says that if something is accepted in this system to be true, then something else can be accepted in this system to be true". More specifically, "If p had been true, then s would be true" is interpreted as follows: "A system S based on p, q, . . . , q_n as axioms and R_1. . . , R_m as rules of inference is consistent and contains the statement s. There is such a statement r which added to S gives a new system $S_1 = S + r$ which is inconsistent. But by removing p from S_1, we obtain $S_2 = S - p + r$, which is again consistent." If r is known to be true, p is a "fiction" and "we have a proper contrary-to-fact conditional". If not, either p or r may be true, but not both; they are "competing hypotheses" and we have a "neutral-to-fact conditional".[1]

Apparently r is introduced and manipulated merely to provide for the falsity or possible falsity of p, so that the conditional statement will not turn out to be true even though we treat it as true initially. In effect, Hiz makes the statement "If p, then s" without incorporating in it any indication of the truth or falsity of p or s, thus relieving it of a perplexing feature of the usual counterfactual; and subsequently, he indicates the falsity of p from without, by introducing as true another statement to contradict it. Before taking this step, however, he treats p and s as asserted, postulating a system which contains both. The relation between p and s, in other words, is that of containment

[1] Henry Hiz, "On the Inferential Sense of Contrary-to-Fact Conditionals", *Journal of Philosophy*, vol. xlviii, No. 19 (13th September, 1951), pp. 586-587.

in the same system, *i.e.* a logical relation; but since the counterfactual character of p requires that we contrast this system with another, from which p, at least, is excluded, we must state that relation from outside the system, in a "meta-language". Incidentally, Hiz seems to forget about s when he changes the system. Logically this is permissible, as s may be either true or false if p is false; but in the usual linguistic meaning of counterfactuals, it should be excluded along with p.

This analysis presents an effective means of incorporating counterfactuals into a logical system and hence of subjecting them to ordinary logical operations, including that of material implication. It is noteworthy, however, that in order to do so, we must leave an important feature of such statements outside the system. That feature is precisely their counterfactual character, since within the system we have merely the assertion of p and the assertion of s. One of the exasperating habits of counterfactuals is just this one of shuttling elusively in and out of "the system" into the domain of empirical truth and falsehood, as recognised by all the discussions that find it necessary to refer to induction. What is left over here is the distinctive meaning of "had been" and "would be" in "If p had been true, then s would be true", a meaning all the more in need of explanation if we use a logical device which replaces both by the innocuous "is" for manipulative purposes.

In this respect, Hiz's proposal is merely the one briefly considered by Goodman, when the latter notes that his own example of a counterfactual, "If k had been under suitable pressure at time t, then k would have bent", can be restated to read, "If the statement 'k was under suitable pressure at time t' were true, then the statement 'k bent at time t' would be true". Recognising here the advantage of providing two simple indicative statements amenable to truth-functional analysis, which indeed is just what Hiz does, Goodman nevertheless notes that the truth-value of the counterfactual statement itself, being independent of that of the components, is still undetermined (p. 40). Even the criteria for its determination are still undetermined—surely the crucial problem.

Hiz provides a partial explanation of the relation between antecedent and consequent in such statements, when he calls p an "axiom" and s merely a "statement" of the system which contains both. It seems that he wishes to indicate the logical derivation of s from p according to the "rules of inference" which he also mentions. This is made clearer by Chisholm's amplification of Hiz's analysis, identifying the antecedent of a

counterfactual as an "assumption" or "supposition" which, "taken with certain other statements already accepted", will "logically imply the consequent".[1] The important point here is that the "if . . . then" relation expressed by the counterfactual is conveyed by the expression "logically imply", which is the expression of modal implication. It is so conveyed, however, in the metasystem only; within the system we have only a material implication.

Yet this situation requires further explanation if it is to clarify rather than confuse. For we have already noted, as do both Goodman (pp. 16-17) and Lewis,[2] that the antecedent of a counterfactual, taken in isolation, does *not* "logically imply" the consequent; and a metasystematic statement that it does cannot make it do so, but is merely mistaken. The key lies in the "other statements already accepted", mentioned by Chisholm, in conjunction with which the antecedent must be "taken" if the consequent is to be logically implied. These are the "$q, \ldots, q_n$" of Hiz, which at one point he calls merely "axioms" of the system S, but which at another point he identifies as usually including "the whole of our knowledge—with the exception of r" (pp. 586-587). It is the nature of these statements, not discussed by Hiz except in this rather cryptic and inconclusive remark, that ultimately becomes the focal point of all thorough discussion of counterfactuals, and indeed of modalities also. For modalities these statements are the axioms of logic alone, and for counterfactuals they include some additional statement or statements extremely complicated to identify. On the nature of the additional statements depends the sense in which counterfactuals are true even though they are neither tautologies nor statements of empirical fact; and also the sense in which the required metasystematic statement about them, namely that their consequents are logically implied by their antecedents, can be regarded as valid.

One might say, then, that Hiz explains everything about his counterfactual "If p had been true, then s would be true", except the meaning of "had been" and "would be", the meaning of "if . . . then", and the meaning of "true". He appears nevertheless to provide a sound basis for applying logical operations to counterfactuals, one that may well be useful and enlightening in the context of some additional understanding of

[1] R. M. Chisholm, "Law Statements and Counterfactual Inference", *Analysis*, vol. 15, No. 5 (April, 1955), p. 101.

[2] C. I. Lewis, *An Analysis of Knowledge and Valuation* (LaSalle, Illinois, Open Court Publishing Company, 1946), pp. 212 ff.

the problems which he has left over. To obtain such a context, we shall begin with some consideration of modality, which his analysis shows us is sure to have some role.

It is important to observe that Hiz's entire analysis of the "if . . . then" relation in counterfactuals might apply equally to the purely modal "if . . . then". The metasystematic entanglements which he reveals seem inseparable from all modality as such, whether or not there is any involvement with counterfactuals. This situation is made particularly clear by Reichenbach, who interprets both modal and counterfactual statements as "nomological", *i.e.* as expressing relations which are in some sense "necessary", differentiating them as analytic and synthetic nomological statements respectively. A statement of a "connective operation", one in which the relation between components is nomological, is analysed into a truth-functional formula plus a statement in the meta-language to the effect that the formula is necessary.[1] That is, using the "accent" to indicate a connective operation, Reichenbach asserts:

$$\text{“}(x)[f(x) \grave{\supset} g(x)] = Df \begin{cases} (x)[f(x) \supset g(x)], \\ \text{‘}(x)[f(x) \supset g(x)]\text{’ is a nomological formula” (p. 377).} \end{cases}$$

The relation is *logically* necessary if and only if the nomological formula is a tautology, in which case the connective operation based on it is an "analytic connective operation" (p. 355). To state a relation of logical, *i.e.* modal, connexion, therefore, we must state the connexion in "adjunctive", *i.e.* truth-functional, terms, as in material implication, and add the metasystematic statement that it is tautological.

Such a connexion enables us, as an ordinary non-tautological material implication does not, to deduce the consequent from the antecedent. The rule of inference, Reichenbach reminds us, authorises us to infer the truth of b from the truth of a only if the implication $a \supset b$ is also asserted as true. But the latter assertion, in terms of material implication alone, can be made only if the truth of the consequent, b, is already independently known; and in that case its use for inference from a to b is superfluous. If we want to start with the truth of a alone, and infer from it the otherwise unknown truth of b, the implication $a \supset b$ must previously have been established by some other means. This is possible within the limits of the system if and only if we can derive it tautologically from the axioms, in which

[1] Hans Reichenbach, *Elements of Symbolic Logic* (New York, Macmillan, 1947), p. 384.

case we have a modal, or " analytic connective ", implication, one that is always true independently of the truth or falsehood of the components. Such a relation, then, provides the only authorisation for deductive inference from antecedent to consequent. All the operations and results connected with it occur within the system of material implication, although the indication that it is a modal implication does not (pp. 65-66). Thus any indication of modality, such as Reichenbach's " accent ", is, as he would probably have agreed, a metasystematic symbol.

The only possible alternative to the use of a metasystematic indication of modality, either verbal or symbolic, is the one adopted especially by Lewis and Langford, the construction of a complete system of modal logic. It will be noted, however, that the relation of " strict ", *i.e.* modal, implication, around which their system is built, is introduced specifically to express " *pIq* is a tautology " (p. 244, using " *I* " to symbolise material implication), and for the very purpose that Reichenbach's " analytic connectives " have been seen to fulfil—" to provide that canon and critique of deductive inference which is the desideratum of logical investigation " (p. 247). This means that the entire modal logic is in effect a formalised metasystem for deriving and identifying the tautologies of the truth-functional system.

The choice, then, whether to employ the formalized metasystem, a metasystematic symbol of modality added to the system itself, or a mere verbal reference to tautology when necessary, involves radical differences in technique but no real disagreement as to the nature of modality or deducibility. " When *pIq* is true but not tautological ", Lewis and Langford point out, " *q* can be deduced from the two premises, *p* and *pIq* ; but only because ' (*p* and *pIq*)*Iq* ' is a tautology. When *pIq* is a *tautology*, *q* can be deduced from *p* " (p. 244). Actually, it might be still more enlightening to say that *q* is still deduced from " (*p* and *pIq*)*Iq* ", *i.e.* deduced according to the rule of detachment ; but that the truth of the tautological *pIq* in the antecedent, since it is a matter of logic alone, can be assumed without special mention and without prior knowledge of the truth-value of *q*. In other words, " *q* is deducible from *p* " is elliptical ; we should say " *q* is deducible from *p* and the axioms of logic ", as indeed Lewis and Langford often do.

This analysis clearly suggests and amplifies that of Hiz, to which we are now ready to return. We shall want to consider it first in the special case where it becomes an analysis of modal implication pure and simple, without reference to counterfactuals as such ; and we can now see that this will be the case

in which the "axioms" designated by Hiz as "$q, \ldots, q_n$" include only the axioms of the system of logic that we are using, and we wish to add only the hypothesis p. In such a case, when we say with Hiz that "if something is accepted in this system to be true, then something else can be accepted in this system to be true" (p. 586), we are saying that, assuming the system of logic, if p is true, s is *necessarily* true—or that $p \supset s$ is a tautology of the system—or that s is deducible from p and the axioms of logic, although we may neglect to mention the axioms of logic because they are always assumed. In terms of this analysis, whenever we translate an ordinary linguistic expression into logical symbols in order to infer its logical consequences, we are in effect adding it to the system of logic as an additional axiom and deriving the new theorems that this makes possible.

To take a stock example: in proving that if there is a son there is a father, we are adding the symbolisation of "there is a son" to the axioms of the predicate calculus and showing that the corresponding expression of "there is a father" is now deducible (*i.e.* a theorem). We are not asserting that it is a theorem of logic, but that it is a theorem of our temporary system. Nor are we asserting that there is in fact a son, but merely that the assertion "There is a son" is treated as true for the purposes of our temporary system; and this is just what we mean by calling a statement an "axiom". If we tend to believe the more permanently adopted axioms as in fact "true" more than we do this one, that is another matter, and one entirely external to the system. But if at any time we do wish, for empirical reasons also quite external to the system, to assert that there is a son, we are justified in inferring that there is in fact a father also, although we do not know that fact by experience. That is, we may assert the implication although we do not previously know the truth-value of the components; and if the antecedent is also asserted, assertion of the consequent is validated by logic.

So much for the modality involved here. It now becomes obvious that if there is in fact no son, we are justified in saying that "If there *were* a son, there *would be* a father", because the truth of a tautology is independent of the truth not only of the consequent, as in the example above, but of the antecedent as well. We might have called this a counterfactual statement, if it were not a violation of the definition adopted at the outset, which excluded tautologies. In any event, it is a counterfactual in its relation to fact, and points the way to an understanding of the more complicated sort of counterfactual. It is Reichenbach's analytic nomological statement, so interpreted as to assist us, we

may hope, in understanding his synthetic nomological statement; and it is Lewis's tautological "pIq", preparing us for the case in which only "$(p$ and $pIq)Iq$" is tautological.

Now we may turn Hiz's analysis to a second case, where, besides the hypothesis p, we make other additions to the axioms. Suppose these are more general and inclusive axioms than "There is a son", new axioms which make possible the derivation of a much wider variety of new theorems, not merely of one theorem of the form $p \supset s$. This is just what happens when the axioms of mathematics are added to those of logic. Since we want to keep the mathematical axioms on hand to apply to a variety of hypotheses, we include them, together with the axioms of logic, among the axioms designated by Hiz as "$q, \ldots, q_n$" instead of including them in p, and introduce as p any hypothesis that can be handled by mathematics. We now derive s. Does this mean that $p \supset s$ is a tautology, a modal or necessary statement; that s is deducible from p? Yes, if we regard the axioms of mathematics, added to those of logic, as constituting a new, expanded system of logic, so that, when we call $p \supset s$ a tautology, we mean a tautology of this *new* system; we mean that s is tautologically inferable from p and the axioms of logic *and the axioms of mathematics*. If, however, we continue to speak in terms of the original non-mathematical system of logic, we cannot now call $p \supset s$ a tautology, because it is not derivable from logic alone; we must, instead, call it a hypothesis; but "(p and the axioms of mathematics) $\supset s$" is a tautology, *i.e.* a modal implication.

This type of modality cannot be restated as a counterfactual, like our first example, because it has been derived in terms of abstractions and has no relation to fact at all. However, as soon as we adopt an empirical interpretation or application for the mathematical system, so that empirical statements can be substituted for the components of the mathematical tautologies, we have valid counterfactuals whenever we assume that these empirical statements are false. This is the case everywhere from the familiar "If I had four apples" problems of elementary school arithmetic to the most complicated calculations of mathematical physics. We now have, therefore, the structural scheme for the case in which Hiz's statement "If p had been true, s would have been true" represents a genuine non-tautological and empirically relevant counterfactual.

Finally, we come to direct examination of such a case. To take one of Goodman's examples: "If that piece of butter had been heated to 150 degrees F., it would have melted." This

differs from the case of "If there were a son, there would be a father", because it is not a tautology of the system of logic; we cannot, by adding p, *i.e.* "That piece of butter is heated to 150 degrees", to the axioms, infer either s, *i.e.* "That piece of butter melts", or $p \supset s$. The two alternatives, we must keep in mind, are interchangeable, since if we can infer one we can infer the other. *To make s deducible from p, then, we must establish $p \supset s$ in the system* by some means other than derivation from the axioms of logic. There is only one such means available; *we must add to the axioms of logic some new axiom (besides hypothesis p) such that $p \supset s$ will become tautologically derivable from all these axioms together.* The method, in other words, is precisely the one used for mathematics, although it is less obvious in the present case because obscured by factual considerations.

We are now dealing with the crucial point on which the analysis of counterfactuals is generally recognised to turn. It is the point at which they are seen at once to resemble modalities by their necessary character and to differ from them by their non-tautological character. It is here that we shall have to account, if we can, for the statements called "lawlike" by Goodman and "synthetic nomological" by Reichenbach, required for the justification of counterfactuals. It is the intent of this discussion to show that, in the sense in which counterfactuals are necessary, they are tautological; what we need is the right *axiom* to make them so.

Of what sort, then, is the axiom that we need? This is the central problem. We might at first think of using $p \supset s$ itself, p for "That piece of butter is heated to 150 degrees" and s for "That piece of butter melts". This, however, will not do, because we are then merely adding to the axioms the truth of p *and* the truth of s; so that there is nothing left to infer, no modality, and no basis for a counterfactual. In other words, the well-known obstacles in the way of basing a counterfactual statement on a material implication hold, here, even though the material implication is regarded as an axiom.

But suppose we add instead a generalised form of $p \supset s$, a statement not about *that* butter but about *all* butter: "If butter is heated to 150 degrees, it melts." In itself this statement is still, as Lewis argues, a material implication, with all the attendant difficulties (*An Analysis of Knowledge and Valuation*, pp. 217-218). But the statement about "*that* butter", $p \supset s$ above, is now tautologically derivable and therefore states a modality, or theorem of the new system; it is true whether or not the butter is heated. When we now add the assertion of our

hypothesis p, that the butter *is* heated, as still another axiom, we may infer s. At this point we may formulate the situation just as we did in the case of mathematics. While $p \supset s$ is not a tautology, since it requires an axiom in addition to those of logic for its derivation, " (p and the additional axiom) $\supset s$ " is a tautology, enabling us to infer the truth of s from the truth of p as a matter of logical necessity, whether p is in fact true or false—that is, as long as we employ the axiom in question.

This analysis dissipates the problem often raised, that the general implication about " all butter ", introduced as our axiom in the above example, is itself interpretable only in terms of counterfactuals, since the material implication does not exhaust its meaning; so that the interpretation of counterfactuals in terms of the general implication appears to be circular. We can now see that the general implication does not itself state a counterfactual, or anything more than a material implication in general terms; but since it is general, many individual statements are logically deducible from it; and these, since they *are* logically derived when the general statement is assumed as an axiom, represent modalities and may be translated into counterfactuals. The so-called counterfactual meaning of the general implication, then, is simply the counterfactual meaning of any one of its logical derivatives; and the circularity involved in interpreting one in terms of the other is merely the circularity of any logical system, and is not only legitimate but inevitable.

Indeed, it is perfectly feasible to restate a counterfactual in such a way as to express its meaning without the use of counterfactual terms. " If that piece of butter had been heated to 150 degrees, it would have melted " has as its precise equivalent " The statement ' If that piece of butter is heated to 150 degrees, it melts ' is true whether or not that piece of butter is heated to 150 degrees; and that piece of butter was not heated to 150 degrees." In other words, the *equivalent* of a counterfactual, like the equivalent of a modal implication, is a statement of material implication plus a metasystematic statement that this implication is a tautology. The *justification* of the counterfactual, however, requires that the material implication actually *be* a tautology, and therefore is provided by the axiom that will make it tautological.

Naturally, we do not pull such an axiom as " If butter is heated to 150 degrees, it melts " out of thin air to set up an arbitrary system or to validate an arbitrarily chosen counterfactual statement, as we may seem to have done in our illustration. We set up a system of this sort with the intention of

claiming some kind of empirical relevance for the physical axioms and empirical truth for the conclusions; and the axioms must be obtained accordingly. Consideration of the manner in which it is done, however, may be omitted here—a fortunate result of the present approach; and we may merely say that we add the axiom about all butter if it can be properly made available. If it cannot, statement of a counterfactual which presupposes it would be very rash.

This consideration may prepare us to deal with a case in which the tautological character of our derivation of $p \supset s$ may seem to break down. As a tautology of our system, it is supposed to hold whatever may be the truth-values of its components. This property is safe from challenge as long as we stick to counterfactuals; but suppose we discover a case in which p is *true* and s false—a piece of butter which is heated to 150 degrees but does not melt. This is the one case that falsifies $p \supset s$, and in our logic it should be impossible; but our logic does not determine the behaviour of butter. We hope, rather, that the behaviour of butter has determined the choice of our axiom; but it is well known that all such determinations are fallible, and we are concerned now with the *logical* situation that arises when our presumed tautologically derived formula is confronted by its contradiction in empirical fact.

First of all, we should try to find some explanation, some conditions other than the temperature which might affect the melting of butter. The formidable difficulties of determining on a logical basis just what conditions are relevant have been thoroughly aired by Goodman (pp. 17 ff.), and we may observe that he regards as unsatisfactory the suggestion which we have noted in Hiz's analysis, that they may include the whole of our knowledge except the contradictory of the antecedent of our counterfactual (p. 235). Whatever the relevant conditions may include, however, Goodman points out that the distinction between the law itself, which we are now calling the axiom, and the relevant conditions is "imprecise and arbitrary"; that law and conditions might be lumped together, and the relation of antecedent to consequent be regarded as a logical relation (p. 24). He chooses not to adopt this method, but it appears to be a helpful one; for then those conditions would be established as relevant that could be incorporated in other physical laws affecting the melting of butter. These laws would be determined by the method by which our original law, "If butter is heated to 150 degrees, it melts", was determined—an extra-logical method having to do with empirical fact, one which we have

chosen not to discuss but which is obviously that of induction. We can then introduce these additional laws as additional axioms, *i.e.* as additional antecedents of both our modal and our counterfactual $p \supset s$.

If we cannot find any such laws, there are other alternatives. We may be able to re-define "butter" so as to exclude the troublesome instance; and if there are no relevant physical laws already available, this may be simpler than setting out to obtain them. Or we may decide that we are mistaken, and the butter is not really heated to 150 degrees after all; something may be wrong with our thermometer, or it may not work for butter. This, however, involves questions as to the nature and criteria of empirical truth for which there is fortunately no place here. Or, finally, if the law chosen as our axiom, "If butter is heated to 150 degrees, it melts", is not very firmly established, or has no very important ramifications, we may abandon it and cease to use it as an axiom, rather than resort to any of the other alternatives; and if none of the other alternatives should prove feasible at all, we should have to abandon it in any event. We might substitute "If butter is heated to 150 degrees, it *usually* melts" or "it *probably* melts", but this is in effect a new axiom, since we should then have to incorporate the same qualification into all statements derived from it, including counterfactuals.

These complications, arising if we find our presumed necessary implication contradicted by empirical fact, explain the obscurity attending the logical status of physical laws as axioms, and consequently attending also the derivation from them of modal implications, tautological within the system which these axioms establish, which admit of statement as counterfactuals. This obscurity does not plague the analogous case of mathematical axioms, because in mathematics we are interested in the abstract system itself, independently of any interest we may also have in its application. If an interpretation involving empirical facts breaks down because some event contradicts a tautology supposed to apply to it, the interpretation may be abandoned while the system, including its axioms and the factually inapplicable theorem, is still retained for other purposes. There would be no point, however, in retaining as an abstract construction a system of physics inapplicable to the actual world; indeed, the involvement of physical language in such a system would make it virtually impossible to develop a new physics with the required relevance.

This kind of analysis does not attempt to go into the question of where we obtain our physical axioms or why they are

empirically relevant. As far as it goes, however, it is supported by the fact that most discussions of counterfactuals observe the possibility of some such analysis, although they do not seem to regard it as useful. We shall have to consider whether it is useful; and the question is still open, because a very fundamental problem still remains. In respect to its possibility, however, we may point to Goodman's suggestion, already mentioned, that the consolidation of the "law" and the "conditions" may transform the implication stated by a counterfactual into a logical one (p. 24); and to the analysis by Lewis and Langford, also already mentioned, of the case where "(p and pIq)Iq" is a tautology although pIq is not (p. 244); and finally to a more detailed statement by Reichenbach, which may repay attention.

Taking "'q' is derivable from 'p'" as a definition of "q is logically necessary relative to p", by which he means the same as "q is deducible from p" or the modal "If p, then q", he defines "q is physically necessary relative to p" by "there is a nomological statement n such that 'q' is derivable from '$p \,.\, n$'". By "nomological statement" he means a "law" of the kind exemplified by "If butter is heated to 150 degrees, it melts". He then points out that, on the basis of these definitions, "q is *physically* necessary relative to p" can be transformed into "q is *logically* necessary relative to $p \,.\, n$", but adds that it is "not expedient since it is the relation between the non-nomological statements 'q' and 'p' that we wish to characterise" (pp. 396-397).

It is indeed true that this latter relation enters into the crucial problem still unsolved in regard to counterfactuals; but after we see what this problem is, we shall see, too, that the foregoing analysis, corresponding as it does to Reichenbach's suggested transformation of that relation into "q is *logically* necessary relative to $p \,.\, n$", can shed at least as much light upon it as the formulation that he prefers, "q is physically necessary relative to p", and at the same time keep the concept of the physically necessary out of logic.

As a particularly clear example of the phase of the problem which we have not yet confronted, we may adopt another of Goodman's illustrations. It is true, he asserts, that "every person now in this room is safe from freezing", and also that "every person now in this room is English-speaking"; yet, in regard to a certain Eskimo now freezing in the Arctic, we infer that "if he were now in this room he would be safe from freezing, but he would not be English-speaking". It appears, in other words, that the *truth* of a general statement is not a sufficient

criterion of its acceptability as a physical law, or, consequently, of its capacity for " sustaining a counterfactual conditional " (p. 42).

Does this mean that there can be valid statements about " all values of x " which will not be truly assertible about *this* x ? Obviously it cannot mean any such thing, although it seems at first glance to do so. It does not, of course, because " all x " means, here, " all persons now in this room ", and the Eskimo is not now in this room. The difficulty is rather that of why we infer that if he *were* in this room he *would* not freeze, since the generalisation does not refer to him at all ; *i.e.* he is not one of the x's. In fact, our generalisation is utterly useless as an axiom, or physical law, leading to no inferences and therefore to no counterfactuals, because all values of x are known. In Goodman's language, it is " exhausted " (p. 92).

Suppose, then, we drop the too restrictive word " now ", leaving as our axiom " Every person in this room is safe from freezing ", or " If any person is in this room, he is safe from freezing ". We might also try, for that matter : " If any person is in this room, he is English-speaking." This makes it possible to add to the x's already known, by bringing additional people, including the Eskimo, into the room. Or, to avoid the complications associated with the notion of making " possible ", suppose someone *does* enter the room ; we then find that he is a legitimate x in terms of our new general statements. In terms of anything taken into account so far, we can now attempt to apply our two companion axioms, although the second may seem strange to us.

Now, however, suppose we find that the newcomer does not freeze, but also that he does not speak English. We can retain the first axiom, but must abandon the second, because the necessary, or tautological, inference that *this* person speaks English is empirically false. As we have seen, there are other alternatives in such a situation. But it would hardly be feasible to re-define " person in this room " ; and if we handle the situation by finding other conditions to explain why the man does not speak English, we shall find that the circumstance of being in this room soon becomes superfluous.

Obviously neither science nor common sense operates in this careless and cumbersome way. We do not select " axioms " at random and maintain them until their consequences are falsified by events. On various grounds, we expect in advance that some general statements which, if taken as axioms, will lead to inferences about things now unknown or events that have not yet occurred, are very likely to be falsified by some of these

things or events; and we do not bother to adopt them as axioms if we expect soon to be compelled to drop them. The grounds on which we adopt them, however, have little bearing upon the fact that, once we do adopt them as axioms, and as long as we retain them as such, any implications derived tautologically from them are modal implications, logically true in terms of the given system, independently of the truth-value of their components, and therefore admitting of statement as counterfactuals. It is logically possible to set up an axiomatic system from which we can infer that "If Eskimo *a* is now in this room, then Eskimo *a* speaks English" is a tautology of the system, independent of the truth-value of antecedent or consequent; and the corresponding counterfactual, "If Eskimo *a were* in this room, then Eskimo *a would* speak English", would follow. The axiom making this inference possible, however, would also lead to the same tautology about the next person that *does* enter the room, and the next, and so on; and since we expect one of these cases to contradict it sooner or later, making the whole system inapplicable to the empirical world in which we live, we discard it in advance of such contradiction, or rather we do not bother to set it up in the first place.

Although the grounds for such expectation extend beyond the purely logical problem of counterfactuals, some mention of the questions into which they do extend may be made here. As is already obvious, some physical laws are not themselves axioms but are derived from axioms—*i.e.* from more general physical laws or theories of which they are logical consequences. These derivative physical laws are therefore themselves modal implications, just as the individual statements expressed as counterfactuals are modal implications, and for the same reason. This circumstance is responsible for much confusion. It leads interpreters to see before them either an endless hierarchy of counterfactuals or an endless hierarchy of modalities, or both, impossible to interpret in any other terms; *i.e.* the law by which we explain a counterfactual turns out to be another counterfactual, to be explained by another law, etc. That there is no limit to explanation, that in fact science does try to derive every law from a more general law, is surely true; but that this makes it impossible to translate the counterfactual "if . . . then" into other logical relations is not true, because we do ultimately reach a law that at a given time is not a logical consequence of anything, but from which other laws and ultimately individual counterfactuals are inferred as logical consequences; and this kind of law is itself neither a counterfactual nor a modal statement.

This kind of law, in other words, is precisely an *axiom*; and every physical law turns out to be either an axiom or the tautological consequence of a physical axiom plus the axioms and rules of logic. The physical axiom need not be a very sweeping one; and on the other hand, a very comprehensive law may be non-axiomatic if it can be derived from a still more general one. If it is derivative, but not if it is an axiom, it is a modal statement, as Reichenbach indicates; not, however, because it has a property of " physical modality " which is different from logical modality, except in the sense that its logical modality, *i.e.* its tautological derivation, depends not only upon the axioms and rules of logic but also upon the addition of a certain other axiom or set of axioms, physical in character. But the axioms themselves are not nomological in this sense, although they may be called nomological in the sense that they *establish* laws. They are not necessary consequences of the system, because it is they that establish the system; and indeed they are not necessary at all, because there can be no necessity or modality in logic other than the kind that consists in being a tautological consequence of a system, and we have found that a special property sometimes called " physical necessity " is merely logical necessity based on physical axioms. The axioms themselves, moreover, may be interpreted as material implications and still have all the required consequences.

The method by which the axioms are obtained is in this way prevented from complicating the logical problem. That method, which we have avoided discussing until now, is either logical construction or empirical induction; and both involve problems, not the least of which is the interrelation of the two. Both lead to reasonable expectations as to the more or less permanent applicability of an axiom, and these expectations determine what systems we set up as the basis of counterfactuals. The fact that, if we choose amiss and derive a " tautology " which is falsified by an event, this mishap is itself part of the inductive process determining further axioms, is another reason why discussion of counterfactuals becomes tangled with the discussion of induction. Yet it seems that the *logical* justification and analysis of counterfactuals is possible without analysis of induction.

Considerations as to induction and other methods of obtaining axioms and laws may, of course, enter into the *semantic* justification of counterfactuals; and the explanation of the sense in which they are true in some empirical respect is no simple matter. But counterfactuals are not alone in being so stranded in mid-air; for nothing in the present discussion has established any kind

of statement whatever as true in any such sense. That is quite another problem; in the case of counterfactuals it is a problem concerning the empirical applicability of logic. We may neglect it, then, and still conclude that counterfactual statements can be justified only by modal statements, *i.e.* by statements of an "if . . . then" relation established independently of the truth-values of the components; and that modal statements are justified only as tautologies of some system. The system, however, in order to include genuine counterfactuals, *i.e.* counterfactual statements whose components have empirical content and whose consequent is not tautologically implied by the antecedent, cannot be that of logic alone but must be expanded by appropriate physical axioms.

Short Hills, New Jersey

IV.—THE LOCATION OF SOUND

By Brian O'Shaughnessy

1. *Introduction*

I can hear a clock ticking but I have no idea where the ticking is coming from. Does this mean that 'I hear the sound, and it seems to be nowhere' or does it mean that 'so far I only hear the *sound*'?

This problem is the theme of the earlier part of the paper. Very shortly I will bring out how it is that this problem relies on and gives expression to a basic puzzle concerning the location of sound; that puzzle then develops into the main topic of the paper.

We could put the above problem about the clock in another way. That a sound is coming from the right is something that I can hear, even though it is not itself a sound. So if the sound changes locality then my auditory experience changes.[1] For 'I can hear that it is coming from the right' must be said to contrast with 'I can hear that it is coming from the left', 'from above', etc. Suppose as I am listening to a ticking sound coming from the right that there is a change; suppose I say 'There was a change just then. I can still hear that sound only now I have *no* idea where it is coming from.' Well, I still have the experience of the ticking. Then have I gained a new auditory experience in place of 'it is coming from the right' one that could be called 'the experience of its coming from nowhere', *i.e.* do I have the experience of the sound as well as the experience of its coming from nowhere? Or do I have the experience of the sound and that is all? Have I lost something and gained something in its place, or have I just lost something?

2. *Do we experience the sound as deprived?*

(*a*) When I hear a sound and have no idea where it is coming from, am I hearing the sound and having the experience of its coming from nowhere? Or am I just hearing the *sound?*

I maintain that there could not conceivably be an experience of 'hearing that a sound comes from nowhere' nor of 'hearing that a sound comes from somewhere' whereas clearly there is

[1] Later on, towards the end of 4 (*b*), we will assess this remark.

such a thing as 'hearing that a sound comes from the right'. I will here briefly summarise my reasons. First of all it makes no sense to say of a sound that it comes from nowhere and therefore it makes no sense to say of a sound that it comes from somewhere; it follows that we cannot speak of hearing that a sound comes from nowhere or from somewhere. Secondly, supposing there to be a sound which does not *sound* as if it comes from anywhere no matter how long we listen to it, then if someone heard this sound for an instant and said 'I can hear unmistakably that it is that sort of sound' then we would not understand him; not even if he was always right. Had he conceivably been able to hear such a thing, that experience would be as good a candidate as any for the title 'hearing of a sound that it comes from nowhere'. But such an experience just cannot be.

It would be wrong to conclude from this that I believed the answer to the question set out at the beginning of this paper to be that 'I am hearing only the *sound*'. For that answer is altogether metaphysical in character, and first and foremost—before acceptance or rejection—it calls for philosophical analysis. I will come to grips with the question it is supposed to answer, and so also with the answer itself, by showing how it arises out of a certain puzzle and by subsequently analysing that puzzle.

The puzzle is this. How is the sound which we hear related to whatever else it is we hear when we hear that it is coming from the right of us? I will expand this statement of the puzzle by describing the two conflicting answers we are drawn to give to it; the first is as follows. When I hear that a sound is coming from the right then what I hear is other than and additional to the sound which I can hear; it must therefore be the sort of thing which one could conceivably remove or erase, by attention perhaps, leaving in its stead something that is simpler or purer. If that is so, then we should at least be able to describe a case in which we encounter a sound in this state; and surely it will be the case in which we hear a sound and have absolutely no idea where it is coming from. So surely in this case I would have to be described as hearing the sound itself (only the sound). So if we seek to answer the puzzle by saying that the relation is additive, we are compelled to say that the answer to the question we began by discussing is that 'I am hearing only the sound'. This establishes the connexion between that question and the puzzle.

Here is the other side to the puzzle. Against the above attempted solution it may be said that the separation we have

suggested is artificial: if we say that the sound we hear is one thing to which is added something else which corresponds to what we hear in hearing what is its locality, this may be to treat as separate two things which cannot be treated separately without risk of grave damage to either member. It is to treat as a mere complexity what is in fact some kind of indissoluble unity, perhaps of the organic kind. So that if we were to hear a sound and have no idea where it was coming from, we might perhaps feel that such a case was incomprehensible, almost a challenge to reason. Eventually however we will say that we have here an example of sound in an unusual state, a state of deprivation, *i.e.* we cannot say that we are 'just hearing the sound' because that suggests it is in its natural state. So we must experience the sound as deprived, we must have the experience of hearing the sound to be lacking or lessened in respect of locality. This connects our original question with the other leg of the puzzle.

(*b*) I will try to explain the notion of deprivation by means of a comparison. When King Lear first comes upon Edgar out in the middle of the storm, alone, practically naked and seemingly quite demented, he exclaims of him: 'Is man no more than this? Consider him well: Thou owest the worm no silk, the beast no hide, the sheep no wool, the cat no perfume:—Ha! here's three of us are sophisticated!' and adds 'Thou art the thing itself: unaccommodated man is no more but such a poor bare forked animal as thou art.' Compare this to the claim that hearing a sound which seems to have no locality is hearing the sound itself. Now contrast with this the claim that this is hearing the sound in a state of deprivation. This in its turn may be compared to the view that what Lear saw was man in a state of deprivation—which is incidentally surely correct. One could hardly say that man in a state of insanity was man in his natural state, unless the permanent standing conditions of reality appproximated to something like the traditional religious conception of hell. It would be like saying it is of the nature of cigarette lighters that they don't work (which is a wry joke). So the question whether or not we are hearing the sound itself may be construed as asking whether locality has a place in the nature of sound. If, for example, we say we are hearing the sound itself, this says that locality, like the hide of the beast, is so to speak merely borrowed and flung onto the back of the sound; as opposed to the way in which teeth or nails or hair make their appearance on the human body. If we deny this and say we are having the experience of its lacking in locality, this is to say that it is like coming across man in the

deprived state Edgar appeared to be in—temporarily robbed of part of his essence, namely reason.

But surely whether or not we have this experience must be an empirical rather than a philosophical matter? It is, surely, possible that some people will not experience the sound as deprived? So how can it possibly be our concern? Before I can really meet this objection it is necessary for me to point out a very important distinction; it is a distinction between two possible meanings of 'an experience of hearing a sound to be lacking in locality'. We may in using this expression either be referring to the experience of deprivation just discussed, or we may be referring to the alleged experience of 'hearing a sound to be lacking in locality' mentioned at the beginning of Section 2 (*a*).

In everyday speech we may say 'Have you ever read right through the Telephone Directory? It is a nightmarish experience' without wishing to claim that any example of reading *is* an experience, for clearly it is an activity. It is not of the essence of any example of reading that it is nightmarish, whereas it is of the essence of nausea or pain that it is not enjoyable, so that it makes no sense to say 'I dislike nausea/pain'. The first is a matter of fact and the second is a matter of essence; one description is contingent and the other is necessary. Suppose that most people would experience nausea if they were to carry out such and such an action or see such and such a sight; then we could describe carrying out that action or seeing that sight as 'nauseating experiences'. This means there is an ambiguity in 'the experience of seeing x' and suchlike expressions. Philosophers want to substitute for 'x' expressions like 'red' and ordinary speakers will substitute expressions like 'what goes on at the abattoirs'. The distinction I have in mind is that between statements of the form 'experience of x' where x is more or less the description of that experience, and those statements where it is not, or at any rate where the nature of the experience is not entailed by x.

I shall elaborate the distinction between the different senses of 'experience of hearing a sound to be lacking in locality' by giving a more detailed account of the 'deprivation' sense of this expression. This experience which we have been considering could be better described simply as an experience of deprivation rather than of deprivation of *locality*; it may be likened to what we experience on meeting a friend whose smile reveals that a front tooth is missing; and contrasted with the experience we have on seeing the mouth of a baby who has been born with all his teeth but one—a front tooth—already in his head. Given the same sort of visual datum in these examples, we have on the one oc-

casion an experience of deprivation and on the other occasion an experience of excess : in one case the mouth wears a diminished or even withdrawn-into-itself look, in the other case it has an expansive self-seeking almost flamboyant appearance. Notice also that it is not by means of this experience that one finds out what it was one expected. For the experience is *what it is like* to see such a mouth given the context, namely, the expectation, assumed. This experience unlike the earlier example at the beginning of Section 2 (*a*) is not cognitive in character, and that is a very important difference.

Now whether or not we have this experience does I suppose seem to be an empirical matter. It seems conceivable that a strange and deficient sort of person be surprised at the missing tooth and yet never have this experience, so that noticing the missing tooth would for him be like noticing that his comb had a missing tooth. Perhaps this is also true in the case of the sound which seems to lack locality. But even if it is, we do not need to investigate the empirical question, for the different but closely related question I choose to discuss is this. Is there a *basis or explanation* for such an experience in the case of hearing a sound which seems to lack locality as there was in the case of the missing tooth ? This *a priori* question, as I have maintained, takes us right back to a discussion of how a sound is related to whatever we hear when we hear where it comes from.

(*c*) So in seeking to answer the question ' Do we have the experience of hearing a sound to be coming from nowhere when we cannot locate a sound ? ' we come up against a puzzle concerning the relation between a sound and whatever we hear when we hear where it comes from. Now since the rest of this paper is devoted to an analysis of that puzzle, I shall here restate it.

If I hear a sound and it is coming from the right of me and I can tell by listening to the sound that this is so, then, since I tell this by listening to the sound, surely what I hear when I do hear this will be something like another sound—remote and faint, or sharp and clear, as the case may be. But if it is something like another sound, then how can this have anything to do with the first sound ; how does one connect the sounds ? Alternatively, if it is nothing like another sound, then how can I be said to detect it by listening ? Since we cannot be said to hear anything other than sounds, and since the sounds would still need to be connected, we are driven to saying that if I listen to a sound and tell merely by listening that it is coming from the right of me, then what I hear must somehow be part and parcel of the sound. But this solution

which seems forced on us is not really satisfactory either, for surely the sound is one thing and its locality is another ?

I have criticised the view that there could be an experience of ' hearing that a sound comes from nowhere '. If that criticism is correct it would apparently seem to imply that when I cannot locate a sound then I am hearing *only the sound*. That in turn would imply that when the sound seems to gain a locality then something other than the sound has been added to the sound ; in other words, we appear to be committed to saying that what I hear when I hear where a sound comes from is other than and additional to the sound. It therefore seems appropriate for us now to examine this claim, as the other half of the dichotomy.

But how shall we go about it ? If we refer to the actual statement of the two puzzles we have been concerned with in this paper, then we will notice the occurrence of expressions like ' the *sound* ', ' the sound itself ', ' only the sound ', ' other than the sound ', ' part and parcel of the sound '. These expressions are at the root of our difficulties, for each of them is being used metaphysically. It would, however, be facile, even in my opinion dishonest, to say that ' metaphysical expressions have a use of their own ', and what is more it would be just plain wrong.

A metaphysical use is not a kind of use at all. At its best it is a network of possible uses, but this crystallised or telescoped network loses all its power of fascination, once this fact is realised and once all the parts of the network are separated and laid out for inspection. This is what I propose to do to the expression ' the sound itself '.

I shall consider an attempt to introduce a metaphysical use of ' the sound itself ' in *such* a way that the answer to our puzzle would have to be that when I hear where a sound is coming from then I am hearing something other than the sound itself. I shall show that this explanation, while seeming to succeed, actually fails, and I shall try to bring out that there are a number of things or aims which we might have in mind in appealing to the expression ' the sound itself ' and that as these vary so the use of ' the sound itself ' will vary. The moment we define these aims or bare the network of possible uses, it becomes clear that the task assigned to the metaphysical expression—proving that when I hear where a sound is coming from I am not thereby hearing the sound—is quite beyond it. We are witnessing here that curious juggle between ordinary and metaphysical speech which is typical of metaphysical argumentation ; it wishes to have the best of both worlds, to recommend the attitudes or expectations

that follow from certain everyday descriptions, and yet to use the expressions that occur in the description in no ordinary way.

3. *What is it to listen to the sound itself?*

(*a*) What is it to listen to the sound itself?

If one listens to a sound one can be asked to attend to the pitch and volume of the sound; also one can be asked to listen to the sound for locality. Does this division, perhaps, show what it is to listen to the sound itself? So that when I say that I hear that the sound is coming from the right, what I have heard is something other than the sound itself?

One can listen to the warmth in a clarinet passage, or to the regret. Is listening to the regret in a note listening to the sound itself? One is inclined to say that it is not. But it seems closer to listening to the sound itself than is, say, listening to the sound for locality. For at least one seems to savour the sound, to enter into it.

But then, when I attend to the pitch of a sound, perhaps we should say that this is not a case of attending to the sound itself but to one of its features? Well, it may be that there is a case in which I listen to a sound without, so to speak, any of its properties reaching my consciousness. But this case must be very elusive; indeed it is too elusive to be put forward for the post of 'what it is to listen to a sound'; it will be very much a peripheral case. While we are inclined to say 'Only this is *really* listening to a sound' we are also inclined to say that this is hardly listening to a sound at all: if one tries *very hard* to listen in this way one almost moves into a state of black-out or absent-mindedness, one loses the sound altogether.

We might reject this case and turn to the case of attending to the pitch of a sound; and attempt to mark the distinction between this case and the case of listening to the regret in a musical note, along the lines of the philosophical distinction between naturalistic and non-naturalistic qualities. The non-naturalistic qualities, say, follow from the naturalistic qualities *in some way or other*. That is, given the naturalistic qualities of the sound, the non-naturalistic qualities are determined.

But what is it to be 'given' the naturalistic qualities? Is it perhaps to be provided with the readings of some scientific instrument? But they will simply consist in a description of certain vibrations of the air. From these readings one might be able to conclude that a note is probably tinged with regret, and we might construct a machine which said as much. But we could not

hope to feed into the machine the marks by which it is to recognise regret. Regret as opposed, say, to sorrow or wistfulness. If the machine continued to identify regret, then this fact would demand an explanation. Of course there is the additional difficulty of our trying to agree on what are cases of regret. Trying to say what are the marks of regret would be a sort of creative process, rather than a linguistic investigation. Compare with this last point the following point. Suppose we ask the question ' What are the conditions for the use of " nigger " ? ' The answer is that the conditions are one's own corruption on these matters, *i.e.* if a child asks ' What is a nigger ? ' and we hesitate, it is not that we are wondering whether to pass on to him certain information which we possess, *viz.* rules for the use of that word.

I do not doubt that there is a difference between words like ' loud ' and words like ' sombre '. But it is no use trying to put this in terms of a ' sensory substratum '. The question ' What makes that sound sombre ? ' is one that one can only *begin to answer*, whereas the question ' What makes that sound deep ? ' is either not a question at all, or else it is a question of a scientific character. Here is one difference. But it does not follow from this that in listening to the sombreness of a sound one is not listening to the sound itself.

That the sound comes from the corner of the room is something that will not enter into a description of the sound. That it is loud is something that will. This certainly is a difference. So if I am listening to a sound and someone asks ' Is it loud ? ' and I say that it is, then I am reporting on something which enters into a description of the sound. But if I say ' It is coming from the right ' then I am not. So surely in the first case it is essential for me to hear the sound and to attend to the sound itself, and surely in the second case it is not. But of course it is.

(*b*) Listening may appear to be no more than a ritual.

When I listen to the sound for locality I am not trying to pick out a feature of the sound ; yet I listen with care. I am not trying to pick out another sound ; what use would it be if I did ? Would I also pick out the connexion between these sounds ? Yet I say that I locate the sound by listening to it.

One wonders if all of these things can be true. In a way it seems clear that they cannot. Listening—since it is not for anything that can come from anywhere, that can be loud or soft or high or low—may appear to be no more than a ritual, *i.e.* it may seem to be a mysterious fact that having listened we can then say where the sound is coming from. Yet this is one of the ways

in which people generally locate sounds, so how can we describe it as mysterious ? What have we got to oppose it to ?

Can we cast the above statements into a form that will reveal an inconsistency ? Consider the following. ' If I am to tell where a sound is coming from, then it is quite essential that I listen to it ; yet I do not listen for just that sound nor for a feature of that sound, nor for any other sound.' This is reminiscent of the example of the person who claimed to be able to see things in the dark : ' He hears something, and what he hears is not a sound, yet it makes no sense to say that he hears that something and does not hear the sound.' Is that intelligible ?

It involves a misuse of the expression ' hear something '. If I say ' I can hear something ' I cannot complete this statement with ' I can hear where that sound is coming from '. That ' I can hear where it is coming from ' has a use, does not mean it can be preceded by ' I can hear something '. For if we were to treat this as an entailment, then all that ' something ' would mean at this stage would be that I properly used a sentence which began with the words ' I can hear '. We cannot solve puzzles by definitions.

Then is it a different sense of ' hear ' ? That question is almost invariably superstitious ; a form of panic. One cannot alleviate the difficulty in this way. The puzzle will persist in the form ' What is it to hear that a sound is coming from a certain direction ? ' or ' How is it possible to locate sounds by listening to them ? '

(*c*) Summary.

Someone says ' When I listen to the sound for locality I am not listening to the sound itself '. ' As opposed to what case ? ' we say. ' As opposed, say, to listening to the sound for pitch or volume.' Has this made clear the use of ' listening to the sound itself ' ? That is to say, is it clear what we are to say of other cases ? Can we say what we mean by ' The sound itself ' ?

A natural way for the above person to go on would be to say ' Well, the pitch of a sound is something *auditory*, its locality isn't. For anything to be of the sound itself it must be auditory.' Uncomfortable as this makes us feel, it may nevertheless seem to have hit the nail on the head. But, after all, is it clear what the word ' auditory ' means ? The use of this word mirrors a confusion which I attempt to bring out by the question ' What about " warmth ", " sombreness ", " regret " ? ' For at the back of this lies a metaphysical theory of the sensory substratum—the soil from which everything else grows—and a word like ' sombre '

presents serious difficulties for this. Is it part of the sensory substratum? Certainly it differs importantly from a word like 'loud'. Well, then, what is the substratum of sombreness? One cannot really say. One ought to be able to say, and one cannot.

The original opposition has come to grief. One might, however, oppose to 'listening for locality' not 'listening for pitch' but 'just listening'. Now we are thinking along the following lines: 'Don't just listen to the double-bass passages' I say to a person who plays this instrument 'listen to the *symphony*!' It is simple enough to obey this order, but it is a strange business to 'just listen' to a sound. The concept 'just listening' is, one might say, a half-grown concept.

We are here invoking the concept of pure, featureless, listening. But locality is not even a starter in this field: it is not a feature of the sound. If what we mean by 'listening to the sound itself' is shown by opposing the pure, as yet improperly specified, case to the case of listening for pitch, then if someone asks us 'Is listening for locality a case of listening to the sound itself?' how are we going to know what to answer? So if someone says 'No it is not a case of listening to the sound itself' then we do not know what to say about this, we do not know what he is saying. The concept of pure listening or hearing is a metaphysical concept which finds expression in a fairly rarefied case. Compare how the concept of the 'given' finds expression in the following. Someone says something, some distance away, and all I hear is a 'mess' of sound; suddenly in memory this 'mess' shakes into place, a coherent sentence resulting. One taps a paper covered with iron filings, and suddenly—imperceptibly—they reveal a picture of the magnetic field.

Both of these appeals to the expression 'the sound itself' are attempts to get us to think along the following lines. The sound is one thing, say a face, and it has features, namely, nose and mouth. Also it has an indefinite something which hovers around, a sort of light or aura. One has to look at the face to detect the aura; once one has detected it one can give one's undivided attention to it, and in attending to it one cannot help seeing the face, but one is not attending to the face. So then it is said 'In attending to the face's aura we are not attending to the face'. Someone now says 'Attending to where the sound is coming from is not attending to the sound itself'. One thinks 'Yes I understand that'. One asks 'What *is* attending to the sound itself?' and is told 'Attending to the pitch'. A difference between pitch and locality is that pitch enters into a description of the

sound, locality does not. But what follows from this? Listening to the sound for locality is different from listening to the sound for pitch, in that in one case we are listening for something which enters into a description of the sound, in the other case we are not. Now what is the interesting proposition that this entails? We have noticed a difference and we can say what it is, but a set of metaphysical concepts are invoked in an attempt to mark the difference and a further one is introduced in order to say what it *really* is, to say it amounts to the presence or absence of a certain metaphysical entity. This is the point. An expression is brought in 'the sound itself'—an expression with no fixed use, or with a very tame use—to demonstrate that the order 'Don't listen to the sound please, listen to where it is coming from' is a genuine order. Now the speakers of the language do not know when to say this, we do not know how to use the expression, even though there may exist 'proofs' that really we do.

4. *How is it possible for 'I can hear that it is from the right' to make sense?*

Asking the question 'When I cannot locate a sound am I having the experience of its coming from nowhere?' is a way of asking 'When I locate that sound, is what I hear part and parcel of the sound?' and we rejected the idea that there could be an experience of 'hearing that a sound comes from nowhere'. We also discussed the idea that being unable to locate a sound implies that we are hearing the sound itself, and we have seen that the varied differences between words like 'loud', 'sombre' and 'from the right' cannot provide us with a proof that it makes sense to request someone to cease listening to the sound and to begin listening to where it comes from. Having rejected these metaphysical solutions of the puzzle, it is now my purpose to show in rather more detail why we are driven to offer them. I believe that though these solutions differ, they each share a *common standpoint*. What exactly is common to them will emerge after a time in what follows; as it does emerge so will the source of the puzzle become clearer and, to some extent, the puzzle will be understood and broken.

(*a*) There are a number of accounts of what goes on when I hear that a sound is coming from the right. I shall list some of them.

Suppose we were to assume that all that can be heard are sounds. (What can one smell but smells?) Then hearing that

a sound is coming from the right must consist either in hearing part of that sound, or in hearing an additional sound. But if locality were part of the sound, somewhat in the way the tempo or key is part of a melody, then hearing a sound which seems to lack locality would involve having the experience of its lacking in locality. For it could not be described simply as hearing the sound alone. But first of all there could not be such an experience, and secondly it is *just as natural* for a sound to seem not to come from anywhere as for the reverse. Alternatively, if hearing locality is hearing an additional sound, then we have simply added to the number of entities involved ; the two sounds will still need to be connected.

Faced with these difficulties, shall we say that we can hear more than sounds ? So that I would have to have heard a something else ? Compare—smelling trouble. But that would seem to involve us in a curious extension of the auditory sense. Moreover, there is nothing we can do that will explain this use of ' something else ', it has no further use.

So we discard this view and, I think, return to the view that it must be a sound that I have heard, an additional sound. But then if it is an additional sound that I have heard, that sound will need to be interpreted, to be correlated with a particular direction, for its significance lies outside of itself. It cannot, surely, carry its own interpretation.

But now what would it be like to hear what I hear when I hear that a sound is coming from the right, as I must have *originally* heard it, uninterpreted ? Such a state of affairs is, I submit, inconceivable. Moreover, there is no such thing as continuing to hear, or hearing five times in succession, what I heard when I heard where it was coming from. For if I say ' I heard that it was coming from the right ' I cannot be asked ' How long did that last ? ' Whatever it is that I hear when I hear that a sound is coming from the right, interpretation seems to be of its essence. But a sound that is stamped with its own interpretation is a pretty strange sound, we think. Indeed, it is not a sound at all.

When I report that I can hear that the sound is coming from the right, that seems perfectly to describe the ' given '. It is almost as if the mind had spread further than we thought, as if it had seeped out here and there into the world. Supposing that we had decided to say that it was *something* that I heard ; then the one and only description that could ever be applied to this something—and it would fit to perfection—is ' it was from the right '. The utility of the word ' something ' would have shrunk to the size of a mathematical point.

Now these are the alternative accounts of ' I heard that the sound was coming from the right ' :

1. I heard part of that sound.

2. I heard another sound.

3. I heard a something else.

4. I had a something from which, via past experience, I could tell that it was from the right, *e.g.* I felt itchy on that side of my face.

5. I simply knew.

I have, above, criticised the first two accounts in some detail. The third account is, as it stands, capable of almost any interpretation. At the very least account 4 is untrue. And the last account, which is troublesome and sophisticated, would deny a use to ' I listened to the sound and could hear where it was coming from ' and replace it with ' I listened to the sound and then I knew where it was coming from ' used in such a way that it would not be correct to describe listening as the method whereby we located the sound. The point is that ' I listened and then I just knew where the sound was coming from ' is capable of a number of interpretations ; as we try to demarcate the use we have in mind from these other possible uses we might be described as doing little more than giving a description of the use of ' I could hear where it was coming from ' ; this means that nothing has been achieved by the device under consideration.

And so I am rejecting all of these accounts. They all register embarrassment over ' I heard it was from the right ' which is assumed to be defective and in need of salvation. Apart from 1, they rest on the assumption that ' I heard it was from the right ' needs a sense given to it, *i.e.* they say that whereas I can hear thunder and echoes and ringings in the ears, I cannot hear such a thing as where a sound is coming from. Just as I said that we cannot hear that a sound comes from somewhere.

(*b*) Hearing where the sound comes from is not a case of hearing something.

It is not a case of hearing something and neither is it a case in which we must be hearing anything more than the sound. In other words it may be that *all* I can hear is the sound which I can locate. Whereas, for example, if I can see that the cat is on the mat, then I must see the cat and I must also see the mat.

There is no such thing as attending to or picking out what one hears when one hears where a sound comes from. We might of course choose to pick out that sound, but in that case we are doing

exactly what we would do if we were asked to pick out what one hears when one *cannot* locate a sound! Suppose a sound were to switch from left to right, and unmistakably, then we might be told that ' when it switched from left to right something of an auditory nature changed ' and then the order may be given to us to ' attend to what is new in the situation '. It is not clear what this order amounts to. I can attend to the sound, or I can direct my attention to the right in the sense required, *i.e.* just ' hold it before my mind ' as I might if I were informed that within five seconds something peculiar was going to happen to the right of me. But then the right will seem to have nothing to do with the sound, other than happening to be the direction from which it comes to me ; somehow I can do nothing that will connect these things. I would like to be able to forge a connexion between these items, by something like an ' act of thought ', and then to stand back and direct my attention to this structure which I have thought into existence. But this is sheer nonsense, and anyway I do not think or cogitate that the sound comes from the right, I merely hear that it does.

Very little of this is peculiar to sound. For I cannot pick out what I say I saw when I saw that the word ' between ' in this sentence is between the words ' is ' and ' the ', unless of course I were to point to the words which I *must have seen*, nor can I ascribe to it a colour or shape or even a duration. What I claim to have seen is true, just as it may be true that the sound comes from the right ; and whatever can be described as true cannot be described as an entity of any sort. One is inclined to say that when first I looked at those words I had a significant sort of visual experience, I saw a certain something, and that I am simply unable to recapture that experience ; but if what I claim to have seen was *true* why should we describe this as an experience ? If I could see that those three words were juxtaposed a few moments ago then I cannot now seriously look at them to see if they are, any more than I can go searching for my watch if I know it is on my wrist ; and since I saw that they were juxtaposed when first I inspected them, there can be no such thing as ' recapturing the experience I had when I saw that they were juxtaposed ' unless this is referring to something like surprise or to the fact that I might sometime forget how they were arranged.

If what I claim to have seen was true why should we say that my claim is to have had a certain experience ? Such an experience could not be described as persistent, nor therefore as momentary or instantaneous, nor could it be said to be faint or intense, nor could we attend to it, etc. The only grounds for describing

it as an experience are that I can see that the words are juxtaposed, that I can say when I saw that this was so, and that it makes no sense to say that I chose to see that they were juxtaposed. But the presence of these grounds does not entail the presence of the above (missing) grounds. *These considerations bear just as strongly against describing hearing where a sound comes from as an experience.* Before I leave this matter I would like to point out that it is, in the final analysis, up to us to say just what shall and shall not be called an experience ; though it is as well to remember that there is sufficient agreement on the use of this term for there actually to be paradigms, *e.g.* pain.

But in case it seems from all of this that there is no puzzle peculiar to sound—even though I have continued to draw attention to the important and puzzling fact that when I locate a sound I may hear that sound in complete isolation—I shall remind you of the puzzle by the following considerations. Reality might conceivably have been such that no creature of any sort ever at any time located sounds merely by listening to them. This would mean that sounds would be said to have locality rather in the way in which smells have, so that we would make use of criteria like : (i) what produces the sound, (ii) does the sound grow louder as we get close to what we take to be its origin ? It seems conceivable that the immediate auditory method we have of locating sounds be completely unknown. The question I want to ask is this. Would the change be in the world or in its inhabitants ? Would the world be minus something, would the auditory ' given ' be diminished, or is it merely that people would not possess a capacity they in fact have ?

(*c*) In 4 (*a*) we considered a number of accounts of ' I can hear it is from the right '. Each of them was rejected, and I said that this expression had a sense of its own, and that it did not need to be subsumed under any heading other than itself if it was to gain a sense. But I cannot pretend that it is not puzzling that it has a sense, for I think we still want to ask how it is that it has. Now what is it that makes us want to ask this question ?

When I hear that a sound comes from the right, this is non-inferential and immediate. When I see that a cat is on a mat, I do so *in one glance.* But the former case, unlike the latter, appears to lack complexity : there does not seem to be sufficient complexity present for a thought to be constructed. ' Hearing the sound is from the right ' leads us beyond the confines of the sound, but apparently to *nothing* ! We look for something audible corresponding to ' from the right ' in vain. For a necessary

condition of a sense or thought is the presence of more than one item, surely, and at least I can see both the cat and the mat. But in this case there is apparently only the sound. Neither the right, nor the corner, nor I, are sounds. Yet I can *hear* that it is from the right (of me), from the corner, etc. (The word ' hear ' in this sentence is very important.)

The idea that a thought or meaning is a complexity lies at the back of the puzzle. In 4 (*a*) I said that the metaphysical claims that hearing where a sound comes from is either hearing what is ' part and parcel of the sound ' or is, instead, hearing what is ' other than and additional to the sound itself ', I said that they both share a common standpoint. That standpoint is this. Both assume that some sort of split or division in what is heard when one locates a sound, is needed. They differ merely on the actual site of the split: one speaks of the sound as indivisible, like the original conception of the atom, whereas the other locates complexity or divisibility within the sound, somewhat like the present day account of the atom in which the nucleus is said to consist of fundamental particles bound together by tremendous forces. So we can see that the expressions ' part and parcel of the sound ' ' other than and additional to the sound itself ' *are invoked in an attempt to create this complexity.* Once we are wedded to the idea that complexity must be present, we are firmly caught in the puzzle, for we cannot choose between these alternatives. Then what leads us to try to demonstrate the presence of complexity ? As I said, above, it is the idea that a thought or meaning is a complexity. Well, why do we say that it is ?

The sense of a sentence is a function of the words which make up the sentence, for if we alter one word we will generally alter the sense. But only a sentence can assert that something is the case, a word lacks this power, only a sentence can stand for a thought. This seems to say that a thought is a complexity, *i.e.* that it has parts. Now the parts of the thought ' the sound is from the right ' must include something which is other than the sound, and which I hear. But what ? This is a complete deadlock.

Is it true that a thought has parts ? If the thought is as complex as the sentence, if the sentence pictures the thought, can one think through each part of the thought, stage by stage ? But what could it mean to speak of a fifth of the thought ' It is going to rain ' ? Moreover, some thoughts seem to take *no time* to run their gamut, they are over by the time they begin. I say ' I felt a cool wind and I thought " It is going to rain " ' ; that thought

was like a flash of lightning ; the past tense is its natural element. Then how are we to divide or split such a thing ? It seems that if a thought is a complexity then it must be a strange sort of complexity, *viz.* the kind that cannot be dismembered.

At this one might turn to ordinary experience, combing through it in the hope of finding such a phenomenon. But now, think of the question ' When I hear the sound and have no idea of its origin, am I perhaps hearing the sound in a state of deprivation ? ' I think this question shows insight. For it suggests that a thought or meaning cannot be simply dismembered in the way a machine-gun can : if the parts are separated they all but lose their identity, they almost bleed to death. If I look at those friendly eyes for long enough, they eventually become just *eyes*.

It suggests that the phenomenon we were looking for is that slightly subtle example—organic unity. This would constitute a theory of meaning. Meaning is something which I register or ' get ' when I attend to certain combinations of words, understanding them ; it is an organic unity which pervades that whole and it is more than the sum of those parts. As if the dead array of words had come to life, or in the way a group of sounds make a melody. Just as a hand has more or less no expression whereas a face has, so some combinations of words do not result in a sense and some do.

This is an attempt within a traditional framework to do justice to a new and revolutionary outlook, and it is necessarily unsuccessful. Subtlety is often the last ditch stand of conservatism. This kind of problem can *only* be solved by revolutionary measures.

The criticism that ' one cannot dismember a thought ' is too limited ; ultimately it is a retreat into a mystery. One starts by thinking of a thought as a kind of magic lantern slide, of three or four pictures in a row, which shows for a moment on the ' inner screen ' and which accompanies the saying or hearing of a sentence. This is utterly misguided. For we cannot say that the spoken sentence ' x ' *evoked* the thought x in my mind, in the typical case, nor can we say that it was *accompanied* by that thought. If I were to say ' Just as you were saying " It will rain " I was thinking that it would rain ' I would be reporting on a coincidental and unusual state of affairs ; and if I were to say ' As you were describing your complicated thoughts on that subject I was, thought for thought and at the very same time, thinking exactly those thoughts ' then I would either be some sort of lunatic or else trying to insult you. If this is so, then what is meant by saying that the thought lasted as long as the speaking of the sentence lasted ?

Possibly just that I was in my right mind when I heard you say 'It will rain'. The word 'thought' has, as a matter of fact, a somewhat limited application.

As for the organic hypothesis, we know what it is to 'watch a portion of Smith's irritable face' but we have no idea what is to be called 'Attending to part of the thought "It is going to rain"'. Are we supposed to examine the meaning of that sentence, or are we to turn to those *occasions* on which we speak of the thought as coming into the mind? If the latter, what is to count as observation of that thought: what is it to fail to notice part of the thought?

(*d*) "The sound seems to come from the right; later it does not seem to come from anywhere. And so we heard it when it seemed to be inside the environment of the proposition 'the sound comes from the right', and then we heard it when it seemed to be outside that environment. How does this change of environment affect the sound? Something must happen to it, surely. If the sound is to connect with locality, that connexion, so to speak, must fit the sound. And if it merely touches the surface of the sound, the sound—now on a different level—must adapt itself to receive *this* connexion. In this game there is no such thing as *just* touching anything."

These utterances express curiosity and puzzlement concerning the nature of meaning, and its origins. How is it that a group of words can result in a meaning? Whatever happens, we think, they must connect with one another in entirety and yet preserve their identity in doing so! One of the conditions under which a meaning is accomplished, then, is through alteration occurring in the participants. So we expect the sound in its 'de-propositionalised state' to be different—pallid and wan, yet recognisable.

I think this is an attempt to *break through* to the notion of the grammar of a word; it foreshadows that concept. For it is a truth about colour, it characterises colour, that it can persist in time and that it cannot be heard. So in a way any proposition involving colour tells us *something* about colour: that a certain proposition can exist means something of the participants: if it couldn't exist they would be different from the way they are.[1] So how is it that a meaning does result from the bringing together of colour and duration? And why cannot I hear a colour? If anything, the grammar of 'colour', 'hear', are such that this is so. But that is not so much an answer as a way of indicating the

[1] *Cf.* Wittgenstein, *Tractatus Logico-Philosophicus*, 2. 012 and 2. 0123.

character of the truth that I cannot hear a colour. Really there is no answer to the question ' Why cannot I hear a colour ? ' Then suppose we ask ' How is it that " I hear that colour " has no meaning ? ' That might be construed as a question of an historical character, rather like the question ' How is it that the word " colour " has gradually shifted in meaning ? ' But I think we might meet this question by pointing out that there are no agreed on conditions for the use of ' I can hear that colour '.

What is it to break through to the concept of grammar ? It is to see not that hearing the blueness of the sky is ' unthinkable ' but that it makes no sense to say such a thing. Then what is so remarkable about that insight, why could not *anyone* realise that this was so ? Alternatively, when and how did ' It makes no sense ' get into the language ? Is it a philosopher's expression ?

Seeing that it makes no sense is remembering that if a child says ' I heard the blueness of the sky ' we correct him. It is no good trying to clutter this fact up with theory or with conditionals, it is *just a fact* that we do correct him. What is new and remarkable is to consider as relevant what goes on when we teach the use of an expression.

It reminds us that whether a group of words ' make a meaning ' or not, must show in a possible teaching situation. It is a false lead to look for a complex of objects or to refer to a theory of meaning. Or to refer to ordinary experience (' The sound must change when it is disconnected from locality '). Most people who object to some expression in ordinary language do so without examining its use in any detail. As if the Understanding, like a sense organ, were an instrument which enabled us to investigate the Truth in a peculiarly direct fashion.

The example of hearing that a sound comes from the right, has special virtues. It embarrasses the complexity hypothesis of sense, for in this case—unlike that of seeing a cat on a mat—there is hardly even the *question* of complexity. So in an attempt to make the case conform we try to introduce expressions like ' part and parcel of the sound ' ' other than and additional to the sound itself ' ; they will show that complexity is, after all, present. But nothing is solved by this move, for the distinctions we have in mind are already taken account of by the language, and the word ' sound ' already has a use.

Another virtue is this. The one identical sound can easily seem to come from different directions : a sound can change in *any* way and still be coming from *anywhere*. This, on the one hand, makes us say that we must perceive some relational feature of the sound. On the other hand, I hear the sound alone

and then I am able to locate it; the sound does not fit into any auditory continuum; it needs no other sound to be in existence for it to seem to have a locality. The peculiar thing about it is that it appears to wear its spatial 'properties' like a quality. If one pictures space as a vast ocean and a sound as a small sponge, then it is as if the sponge were situated in a vast ocean which it had, at the same time, absorbed.

What is good about this conflict? It does not show that the distinction between relations and qualities is spurious, but I think it is a sort of shock case that reveals the corrupt motives which underlie the use of this distinction. Whereas some philosophers say the world is 'one' and some that it is 'many', there are other more cautious types who disagree and say it is neither, who regard both these positions as too extreme, *cf.* Idealists, Materialists, Dualists. They are afraid that the sound will expand and eat up the Universe; so they determine to map out its confines, to say where the sound and the rest of the Universe meet. But this refined protest still talks the language of 'one' and 'many'. We are doing a very similar thing when we say that the parts of the proposition change but do not altogether swamp one another; that is why I called it conservative.

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V.—EPISTEMOLOGY AND THE PROBLEM OF PERCEPTION

By F. H. George

I

Many textbooks or general books on philosophy, especially those on 'Theory of Knowledge', point to the fact that the philosopher's interest in *perception* is not the same as that of the psychologist or of any other empirical scientist. The philosopher is usually described as analysing perception in so far as it is essential to the foundations of knowledge, especially our knowledge of the so-called external world and of such things as physical objects. Thus philosophers are interested in the question of what is perceived and the reliability of the perception. The psychologist, on the other hand, is interested in how perception takes place.

How seriously are we to take this sort of distinction? Is it more than a mere division of labour? It is the main purpose of this article to examine more closely the relation that exists between an empirical theory of perception and what has been sometimes called the logical theory of perception.

Price [1] has stated the task of the theory of perception to be that of examining the experiences of seeing and touching and also to examine the beliefs that they seem to carry with them, that such things as material objects exist. Price says that the other senses could equally well be examined and it is the relative importance of seeing and touching that is the reason for their choice. He also expresses the characteristic philosophic view that in this basic examination we cannot utilise our knowledge derived from scientific investigations which, being empirical, are themselves dependent on perception. This leads to a *description* of the process which we might temporarily label 'visual perception'. Price actually uses the word 'examine' and this in fact refers us to our intuitive experience and the use of natural language.

It is also part of my purpose in this note to try and show that the attempts, of such epistemologists as Price and Broad,[2] to solve the philosophical or logical problems of perception, are

[1] *Perception*, p. 2.

[2] *The Mind and its Place in Nature.*

based on a confusion in which they create as many problems as they solve.

In taking this position, I shall support a view somewhat similar to that espoused by Lean [1] about the treatment given to perception by Broad, but differing in certain important respects. I hope further to show the inadequacy of any sense-datum language that is tied to Phenomenalism, and show that its privacy makes it something less than a language, and thus useless for communication.

The principal theme, of the contrast of the causal and the logical theory, exhibits the limitations of any such examination when we realise the limits placed on the natural language from which we start, that is itself constructed on the basis of, mainly unconscious, epistemological and ontological assumptions about the world. Nobody wishes to assert that we cannot abstract the logic of an argument from its factual content, or perhaps more precisely, distinguish logical from non-logical truths. Our problem comes nearer to the question whether we can find any criterion of distinction between the formal and the factual.

The accepted distinctions between the logical and the causal theory of perception are our starting point, and the reason for the acceptance of this distinction is seen to lie in the notion that the epistemologist's problem is to produce, not a causal explanation, but a neutral description of all knowledge which would serve as a logical foundation for all of the empirical sciences. An unwanted circularity it is supposed, would arise if appeal were made to the very sciences whose appropriate foundations we seek. Thus all scientific theory is deemed irrelevant to this approach to perception. It seems likely that the whole of the argument suggested by Price and Broad springs from the distinction sometimes made between so-called formal and factual matters. That the distinction is only a matter of degree seems to be a better interpretation of the facts.[2] A result of denying this particular dualism leads, as both Quine and White suggest, to just the sort of merging of scientific and philosophic ideas that this article will urge in the particular case of perception. Alternatively we might regard the argument to be over the methods available to us to justify certain, or all, of the beliefs that we hold.

There are of course many starting points for such investigations and as a result it is possible that what we finish with will be

[1] *Sense-Perception and Matter.*

[2] Quine : *From a Logical Point of View* see especially " Two Dogmas of Empiricism ". Morton White : " The analytic and the synthetic : an untenable dualism " in Linsky's *Semantics and the Philosophy of Language.*

regarded by some as something different from what such epistemologists as Price and Broad have attempted. Indeed we are dogged by value judgements, and lack of agreement over criteria, about what epistemologists *should* be doing.

II

There is a problem that has occupied philosophers and which might therefore reasonably be called the philosophical theory of perception. This is clearly about our *senses* and about sense-perception, which underlies anything that we can subsume under the word ' knowledge '. It is an almost universal assumption that there is an *external world* and that there are *physical objects*. What is the basis for these assumptions and what is their relation to our sensory information ? In discussing this problem I start, if I wish to keep to the simplest terms, by making some statement to the effect that I see something real or see real things, as opposed to cases where I ' see ' something which I do not take to be real. The notion of reality implies that I may be able to touch it and use other senses to confirm its presence ; it has continuity in both my own and other's experiences. I say a chair is real if I can sit on it and if it has certain other properties that may be observed by the tactual senses, etc. We have on some basis to distinguish these two types of experience, and we may try to start, as Realists have done, by assuming the existence of material or physical objects which are independent of the existence of the organisms that observe them. This is an assumption and one which cannot be proved, if proof by direct observation is required. At the same time we may say that we have no reason to doubt the truth of the assertion that there are things that exist independent of us.

I want it to be explicitly noticed now that as we start to describe, rather than explain, the process of perception as introspectively viewed, we are using a natural language that has been built, not upon epistemologically neutral notions, but upon primitive forms of beliefs, or explanations about the world. Thus we may quickly notice implicit assumptions about classifications, and the distinction between the observing apparatus and what is observed, and more seriously the notion that the things that we see are supposed to have properties, such as that of colour, shape, etc. It should also be noticed that in talking of ' describe ' and ' explain ', we are contrasting the construction of a conceptual framework, independent of all causal explanations, with a causal explanation itself.

In this world of primitive assumptions, based on a somewhat primitive language, we start to try and refine our terminology to deal with considerations that are not normally considered. Thus we will try and play safe and introduce a neutral term ' sense-datum ' for what we perceive, where perception is that which our consciousness is aware of. In fact, of course the notion of sensory organs, even sensory faculties, underlying perception, is somewhat inferential, but it is impossible to describe such acts without making inferences of some sort, and this description in fact is couched in terms of a language that we expect other people to understand. Leaving this question aside for the time, it is an empirical fact that theorists such as Price, Broad and Moore, are soon brought to ask rather odd questions about these neutral terms. For example they come to ask : what do these alleged sense-data *belong to* ? are there such things as unsensed sense-data ? what is the nature of perceptual consciousness, by virtue of which we are acquainted with these sense-data ? And so on. Now it seems that in asking these questions, we are going beyond our search for a neutral description and are seeking a causal explanation. At the same time Broad and Price insist that we must rely on direct observation alone and cannot appeal to evidence from empirical data. To take the first question then : how are we to discover to what the sense-datum belongs when all we are directly aware of is the sense-datum ? The Naïve Realists lay themselves open to obvious difficulties with the case of Illusions and Hallucinations, since no conditions of Physicality can be stated that allow us to believe that a sense-datum always implies the existence of an empirical object. The thing to notice however is that it does sometimes do just this. We may be thus persuaded that sense-data are sometimes indicative of the presence of physical objects, by almost any conditions of physicality, and sometimes they are not. There are also other cases where we have every reason to suppose the actual existence of some colour patch, rainbows, shadows, irradiated dust particles and the like, which may not be obviously material objects but which are not necessarily illusory either. Physical evidence, which we are not allowed to introduce into the supposedly logical question could tell us, by virtue of well-validated explanatory principles, exactly what some of these cases involve. However our interest at this stage should be turned to the point that we do not have to discuss the question of ' to what do the sense-data belong ? ' To say that the sense-data belong to the surface of physical objects even where we might agree that physical objects might be said to exist, is to beg the whole point of the neutral

terminology that was intended. If we were permitted to utilise our knowledge of physics, if only for the business of the better describing the perceptual process in a neutral manner, we should soon say that the sense-datum, or we should probably say that the appearance, connected with physical objects was a function of at least a light source, the physical object, and a visual system. In fact, to ask where the sense-datum belongs is seen to be an unreasonable, and certainly a non-neutral, question. Equally unreasonable seems to be the question of whether or not there are unsensed sense-data, which asks whether something which is part of the act of sensing can exist independent of that act. It is obvious when we are armed with the scientific evidence that we are in fact asking whether the word should be used to apply to a property of an object, our retina, visual cortex, or whatever, and of course we know this to be a silly question.

However let us step aside awhile from this approach and consider what Pap,[1] for example, has said on the *philosophical* problem of perception. He couches the problem thus :—

> " Can it ever be the case that what we perceive is identical with the physical object and its physical properties as ' it really is ' ? "

It should be noticed that the monists and dualists would first disagree over the phrase " really is ", since for the monist perceptions are all there is, and for the dualist there is the question of stating the conditions for the reality of an object as opposed to our perceptions of it.

In this last matter, as Lean [2] hints, there is part of the creation of the very problem that the sense-datum theorists construct themselves, by introducing the very dualism they then turn to and start to regard as a problem ; this is true of Broad's, Price's and Moore's versions of sense-datum theory.

Let us now turn to the Dualistic Phenomenalism of Broad. It is sufficient for our purpose here to note that he, like Price, acquires a logical problem, wherein he denies the relevance of empirical evidence in the form of the laws of perspective, and the findings of physiological psychology. Broad sees the logical question as arising over the apparent inconsistency of perceptual appearances. Thus the physical object looks different for various observers and also looks different for the same observer at different times. Broad's difficulties then take on rather a remarkable form as has been well shown by Lean.[1] Broad argues that if we are to accept the commonsense view that the objective

[1] *The Elements of Analytic Philosophy.* [2] *Op. cit.*

elements in perceptions are the parts of physical objects, we are then committed to one of three possibilities : (i) That physical objects both change and do not change their properties. (ii) That we must distinguish the apparent from the real characteristics of sense-data, or (iii) We may deny the view that the objective element really is a part of a physical object. Actually Broad seems to adopt the third view. But this difficulty is manufactured by Broad himself in the first place when he introduced the notion of objective elements of the perceptual situation. As to the other two possibilities : (i) is ridiculous since the basic notion of contradiction is offended ; how could we, from appearances alone, arrive at the idea of inconsistencies, if appearances are all that we are directly acquainted with? Obviously if our languages fail to account for, or give an inconsistent description of appearances, then it is our languages that are inadequate by the Phenomenalist's own showing. (ii) is inadequate since as Price has rightly said, that to introduce ' apparent ' and ' real ' into the compass of our ' sense-datum ' language convention, would be disagreeable in that it would defeat its very purpose. Actually common sense which seems to be relevant to Broad's view has no trouble with this problem, since the appeal to the non-allowable laws of perspective, without the added clarifying facts of empirical science in the form of physiological psychology, clear the matter up immediately ; this sort of explanation is permitted in the causal theory, but not in the logical theory.

Now there are other matters that we should consider that help to explain the extraordinary state of the logical problem of perception. We should argue, as does Max Black,[1] that the very notion of a sense-datum language is an error in the first place. Such a private method of using terminology does not come within the confines of what are normally called languages, precisely because it cannot communicate information about the empirical world. It does not matter at this stage what the nature of the empirical world is, other than that it should involve public designation or description. Ostensive rules are at the roots of communicable signs and the sense-datum language cannot have these as its foundation. It is clear that the rules for the use of ' sense-data ' cannot be given ostensively. Sense-data are necessarily private to individuals, since if you deny their privacy, you deny their indubitability, which has, in fact, been the villain of the whole piece. The search for indubitability is

[1] Max Black—Symposium on Phenomenalism in *Science, Language and Human Rights.* Proceedings of the American Philosophical Association. Eastern Division, vol. I. Philadelphia, 1952.

misdirected in that to have indubitable basic statements is to tie the notion of basic statement to the non-communicable. Thus we find an indubitable element only to discover that it is useless. Yolton [1] has discussed the question of the *given* and pointed to the difficulties experienced by Russell in separating the epistemological given from the psychological given. The simplest solution would seem to have been to drop the distinction altogether, and with it the idea that there are two sources of information open to us: the given and the inferred. The fact is that sense-data are not explicable except in the framework of a public physical object language in the first place.

Now let us return to the general problem that is generated in philosophy. We must proceed with special care at this stage. There is very little doubt that both Ayer and Lean have been correct in drawing attention to the primarily verbal nature of the problem, and if the problem is one of consistent description of the perceptual process, then this is a truism. However in practice our language has, by its primitive and complex historical background, committed us to certain ontological and epistemological assumptions, such that when we introduce words such as ' sense-datum ' and claim incorrigibility for sense-datum statements or for protocols or basic observation statements, as they are sometimes called, we lose claim to the neutrality of our usage. What in any case can be meant by using a term in a ' neutral ' manner ? But above all when you join with Price in asking ' to what does a sense-datum belong ? ', the notion of a neutral terminology seems ludicrous and the danger of hypostatisation looms close.

Do we wish to assert then that there is a basic error in the notion of a logical problem of perception ? Here we come to the main point of this note. Whether or not Broad, Price and Moore have or have not created their own problems is really of no importance. The fact is that we cannot deny anyone the right to approach any problem in his own way and it is possible there is a logical problem of perception ; but this being the case it does not follow that we cannot use our knowledge of empirical science to fill in those parts of the picture that are necessarily incomplete without it. The whole purpose of empirical science is to fulfil just this aim. Another way of stating this last rather elliptical point would be to say that there is a logical problem of perception (and all matters in all linguistic systems) in that it appears to need to conform to certain fundamental logical rules (*e.g.* contradiction,

[1] John W. Yolton, " Linguistic and Epistemological Dualism ", MIND, 1953.

identity, etc.) and that if one's theory is intended to do more than illustrate these principles of logic, it will not involve strictly non-empirical means alone and thus the questions asked by the epistemologists like Price, Broad and Moore are not logical, but empirical questions, which therefore require empirical answers. Indeed in general (as opposed to a particular case) we doubt the very distinction between description and explanation originally used, and this even applies to a conceptual framework as opposed to causal explanation. Indeed the doubt applies to the distinction between analytic and synthetic which lies at the root of the problem.

With reference to the above, it is clear that knowing the empirical facts does not solve the philosophical problem of perception, but a knowledge of the structure of the process we seek to *describe* is obviously essential to our choice of language and our attempt to find appropriate descriptions ; the notions of perspective, for example, do quickly clear up our confusion over ' appearances '. Our trouble has been the deep-rooted belief in certain epistemological priorities and a failure to observe that linguistic description involves empirical assumptions about the world. Thus the language of properties which is so misleading leads to inadequate description because of the inevitable, and as it happens, wrong epistemological and ontological interpretations that we associate with the term. This is all saying that we cannot have a neutral terminology for this or any other job. The marks that mathematical logicians use are neutral (this may be a significant part of the confusion) but these marks have to be interpreted to be of any use to communicate information, and then their neutrality is lost.

If one may hazard a guess at this stage, it may well be that the problem is shown up at this level in high relief because of its basic nature. Price says clearly that you cannot use science, as this would involve an unwanted circularity. Perhaps you cannot use logic for analogous reasons, since the basic level of perceptual description may be the level of our fundamental notions of logic itself. There are in any case various ways in which investigations may be carried out, and it seems, as Quine [1] has said, that there is no reason to give one of them some sort of priority ; there would be no sanction for so doing. Drop the idea of certainty and accept the notion that our knowledge can never be more than more or less probable and we return to the acceptance of the direct relevance of empirical science to philosophy. This self-reflexive aspect in philosophy has always been anathema to

[1] *Op. cit.*

epistemologists, but there seems to be no adequate reason for not accepting it as inevitable. Our language, it seems, would commit us to precisely this, by virtue of the fact that our language is already based on certain empirically founded interpretations. There must be some sort of circularity in all explanatory systems and it is this that we must accept as inevitable.

III

All the above, perhaps especially the last paragraph, may seem particularly unsatisfactory to many, and if the matter were left there, without giving a brief illustration of the relative simplicity of our problem in practice, the cause for dissatisfaction may reasonably remain. Let us therefore consider some of the well-known examples of problems in perception, and give what is a form of semantic solution, abstracted from, but wholly dependent upon, the pragmatic facts.

Before we start, let us note that we must of course have some rule for distinguishing the real from the imaginary. It is a commonplace that we *can* do just this, otherwise our notions of illusion or hallucination would not have arisen. The fact that any particular distinction also depends on probabilities, and cannot be certain, is also commonplace, and since our system is inevitably tied to assumptions we shall make explicit whatever ontological and epistemological criteria seem most appropriate at any time. Our notions of physicality will thus be based upon the latest information physics can give. We will explicitly use the magnifying glass to investigate the magnifying glass and remain explicitly aware of just this fact. In the same way an investigation of the eye occurs in science which depends on the use of the eye. We must always start any investigation on the basis of some assumptions as well as some unanalysed terms.

Let us now consider a penny. There is clearly a difficulty in describing a penny in any language. We say that it is a round, firm, disc-like, brown, etc., coin. We may indefinitely enumerate the appearance of the penny which may vary indefinitely although they will never vary for any one person except in a temporal continuum. We are aware that we are independent observers of an independent physical object which we may expect to vary as the context of the viewing varies. Thus we fully expect to see a penny appearing elliptical, and even as a thin rectangular band when the edge is presented. These simple facts are part of our common experience. Where we have to be careful, is to attribute a clear *meaning* to the words we use, and be in a position

to distinguish a word that records a mere appearance and one that records an appearance *and* also implies a reality. The fact that this is all corrigible is to be noted. We should also notice that the argument should not be wholly limited to the visual sense modality. We are now in a position to assert, as does Ryle,[1] that we may misuse such perceptual terms, even indeed without anyone's knowing it. We may use for the above division 'see' and 'seem to see' and mean to assert physical existence only in the first case. In other words we use 'see' incorrectly if we are not in the presence of some material object, and should instead say 'sense' or 'seem to see'. We cannot reasonably insist as does Pap[2] that since 'sense' is a transitive verb we must sense something; this is to get tangled up in the sort of semantic web that says if there is a word for it, it must be. A ludicrous state of affairs which semantics is precisely designed to help avoid.

To return to our penny, the plain fact is that no one feels that there is much of a mystery in a penny looking elliptical; indeed we should regard it as mysterious if it did not do so under certain circumstances. There should be no mystery about generalisations from experience, except in that they may be inconsistent. The essential point that we should notice here is that it is difficult to distinguish a generalisation from a statement of direct observation. Indeed our whole point has been that there is no way of distinguishing a statement in a public language which is meant to convey only the directly observed facts from a statement which interprets the facts; or again it makes no sense to talk about direct observation statements, if sense-datum statements are what is meant. The sensory element is not, as Ryle has correctly pointed out, the whole of the observation process, thus the notion of interpretation and generalisation is wholly bound up in a public statement. But *only* language can be mysterious on this account. No one regards it as odd that parallel lines appear, from certain angles of viewing, to converge. Indeed as in the case of the penny, we should regard appearing to converge and looking elliptical as essential to the entities that we call 'parallel lines' and 'pennies'.

From the above two paragraphs, as well as the last section, it should be clear that there has been an uncritical use of 'properties of objects' or 'properties' as well as 'material objects'. We are not confused when we regard the process as a triadic

[1] *The Concept of Mind.*

[2] Semantic Analysis and Psycho-Physical Dualism, MIND, vol. LXI, 209-221.

relation [1] within which a variation of any one of the variables changes our ' perception ' or ' sense-datum '. Thus the answer to the question: ' to what does the sense-datum belong ? ' is either (1) that the question is not meaningful in that a sense-datum cannot be rightly said to belong to anything, or (2) that sense-data can be said to belong to a triadic relationship which we call ' perception ', and which is subject to certain inductively founded generalisations such as the laws of perspective and the data of physiological psychology.

We should now consider another well-known example; that of a stick partially immersed in water. The stick will in general appear bent when placed half in water and half in air, or indeed in any two media which differ in their refractive indices. One way of using the word ' illusion ' would now be to say that the actual properties of the stick differ from the perceived properties. We may simply mean that the tactual perceptual cues will differ from the visual ones and this would constitute one sense of the word ' illusion '. We do not, and should not, expect to see the stick as straight. There is in fact no confusion here between the *belief* and the visual appearance. We may say that what causes the stick to appear bent is the different refractive indices of water and air.

The whole matter of Broad's inconsistent appearances is made clear if we heed what the psychologist and physiologist tell us about the complex nature of the processes we are discussing. We may regard this problem on one hand as merely preserving proper distinction between the uses of various terms referring to sensation and observation. Thus the physiologist reminds us that we are referring to complex neural processes that will require the semanticist to maintain careful verbal distinctions, devised with one eye on the states or acts to be described and one on the language used for the description. The well known category mistakes, analysed by Ryle, are essentially those warned against here. The point is that we do not want to draw a hard and fast line between science and philosophy. That is, we cannot discuss merely semantics at any given time, since the language analysed semantically is the language that has already been used

[1] We are not averring in this article that we should assert that statements are about actual physical objects independent of observers; it seems enough that we may talk *as if* this were the case. Neither do we have to aver that all observers sense, see, etc., the same things when they use the same words; this seems probable but is by no means essential to the argument. Our experience may be our starting point and end point as well, but some relationships in that experience are what we are then discussing.

to describe certain non-verbal designata and their more or less complex relations. Thus the distinction is made between a physiologist who performs operations of observation and records these observations in a language, and the semanticist who can analyse these verbal records, but *not* without regard for the designata the physiologist has been attempting to describe. Thus when Ryle starts his introduction to *The Concept of Mind* with the words :

> This book offers what may with reservations be described as a theory of mind. But it does not give new information about minds.

This is the position of the semanticist since he does not perform the operations of observing the designata that the physiologist performs. But he cannot reformulate " the logical geography of the knowledge which we already possess " without reference to the designata described as well as the language used to describe it. Thus he is not investigating merely logical truths as might a mathematical logician ; he is investigating the logic of *and* the clarity and adequacy of factual descriptions. It is indeed obvious that one man can be both a semanticist and a physiologist. During the day he may make observations and note them down, even to the extent of drawing inferences upon the observations in a more general theory. In the evening he may read over what he says with a critical (logical) eye and see if his language and arguments are well-chosen. His decisions as a semanticist will not be independent of his decisions as a physiologist. Indeed his close friends will be surprised to learn that he is a specialist in two fields.

Thus the problem is to decide how to arrange or categorise certain events from the point of view of description. The language has to refer to the empirical data and also be internally consistent. We may elect to say, simply " I see a material object X " and this would be a perfectly good simple convention for many purposes. Thus if I said it when there was no material X there, I should simply have made an error. There may of course be no certain way of finding out whether or not an error has occurred, in just the same way as there may be no way of ascertaining the truth of a proposition ; this will not impair our findings.

There is another use of ' illusion ' which involves the misinterpretation of a ' sense-datum '. We might mistake a distorted for a normal room, and indeed this can be shown to happen. This last example could be the result of the ignoring (or suppres-

sing) of relevant cues, either by the active organism or by the special conditions of the environment, *e.g.* such as when it is specially manipulated by an experimenter. Thus we begin to understand the nature of the problem and realise the inadequacy of talking of a purely *logical* approach. Broad's [1] actual repudiation of empirical data, in the discussion of the laws of perspective, says of the problem of the penny:

> Our question is as to the compatibility of these changing elliptical appearances, however they may be correlated with other facts in the world, with the supposed constancy and roundness of the physical object.

We must agree with Lean that the laws of perspective are in fact directly relevant to just this problem, and Broad's confusion seems merely to rest on a too narrow interpretation of the words ' round ' and ' constant '. To put it briefly a coin which is round, and of constant shape, will appear of various shapes relative to the angle of observation. This is simple and obvious and does not require, or in any way depend upon, a logical solution, any more at least than any solution to any problem whatsoever needs to satisfy certain logical criteria.

At this point we can claim that the so-called logical problem of perception, which is supposed to involve the apparent inconsistencies in appearances, or in the use of sense-datum, is to be regarded as either trivial or a pseudo-problem, or a question merely of linguistic usage which depends on *what* is to be described.

Apart from the above it is however clear that the word ' sense-datum ' has in fact been used in many ways, since the question " Does the ' sense-datum ' exist whether or not it is being sensed?" is, as we have already noted, based on the intuitive distinction made between an object and an observer, and the promotion of the question asking whether we regard the ' sense-datum ' as having an existence in the observer or in the object. It seems clear that we need to know something of the empirical facts before we can find an adequate verbal description of those empirical facts and it does not make any sense to say that this is a purely logical matter.

The appearance of an object, *e.g.* a penny, will be said to be a function of its relation to a light source and an observer. If the light source is allowed to change in any of a variety of dimensions (brightness, colour, etc.) then the appearance or sense-datum of the object will generally vary also. Similarly if the observer changes his position with respect to the object or the

[1] Broad, *Scientific Thought*, p. 235. See also the following pages.

light source the appearance will change. The sense-datum we must remember is supposed to be a neutral word used to avoid saying that the object has changed and yet to indicate that the appearance of the object may have. Thus we can mean that a sense-datum is what we say we see or perceive according to our intended meanings of these terms ; if so, we must remember that our only evidence for having had a correct perception is on the basis of probabilities which are referred to subsequent consistencies as a result of the perceptual categorisation. In short we infer that we are in error about the facts if no one agrees with us about them, if our other senses give us inconsistent information, or if our subsequent behaviour based on our perception turns out to be inconsistent. We may say, if we wish to use words in such a way, that our sense-datum was of a man (say), but the fact is that it was an ape. Or since this may merely be regarded as the giving of different interpretations to the same sense-datum, we may say that to us it was green and to everyone else it was red. But are these examples really different ? We cannot separate the interpretation that we place on some object from the object itself, regardless of words. We find considerable evidence from psycholgy that supports this assertion. There would be no point in ignoring this evidence ; rather we should be sure that our problem of description is facilitated by such evidence. In this general situation we see again that the word ' sense-datum ' has more than one meaning. If we say that it is what the observer merely records, which is claimed by some to be the certain element in knowledge, then clearly it would not do to identify it as belonging to physical objects in all cases. If we are correct in our perceptions, however, we assume that the presence of a physical object (taken in a broad sense as anything reflecting photons of light) was necessary to the perception.

Perhaps the final word on the so-called logical theory of perception is that there are many solutions which are relative to either our ontological or linguistic assumptions. They depend on the assumptions made about the world, and the rules that guide the use of the intended meanings of the terms used. It is in the matter of usage that we have a convention that talks about real objects, and so on. We have a mode of description that attributes properties to objects or things and which has an ontological commitment that wholly and inevitably overthrows the intended neutrality of our descriptions.

The arguments from Illusion depend on our ability to recognise the difference between a *wild* and a *non-wild* perception, which

suggests that we actually know when a sense-datum is not representative of, or caused by, an external physical object. It may, of course, be caused by an external state, but the false interpretation is a function of the internal structure of the organism. Thus a consideration of the broader context of the question almost immediately gives us a solution. This whole study indeed drives one to agree with Professor Boas' conclusions,[1] when he said:

> The trouble with the epistemologists is that they think that knowledge goes on in detachment from the total context in which it occurs, . . .

IV

In this article I have in fact directed my attack against confusions involved in the so-called logical theories of perception, and almost entirely against the users of the so-called sense-datum language. The reason for this is because they, more than any others, have laid emphasis on the logical nature of the problem. My own position is in general agreement with Lean's in condemning the Phenomenalist viewpoint; not only in Broad's, but also in Moore's and Price's forms. Furthermore while I agree with Lean that Broad, and Phenomenalists in general generate the problems they wish to solve, I would like to emphasise also—a point rather neglected by Lean—that their troubles stem from the nature of the incorrigible 'given' from which they start, and from a belief that they can deliver themselves of a terminology under the guise of verbal neutrality that is in fact—and this is especially obvious from the questions that they subsequently ask—committed to a particular epistemological viewpoint or a particular ontology or both. It also seems probable that the wrongly named sense-datum language is in fact only intelligible within the compass of some prior, realist, terminology.

There are many good reasons that have already been mentioned why I deem phenomenalism to be a thoroughly inadequate interpretation of the problems of perception. The sense-datum language may have appeared at first sight to be a convenient form of description, but turns out to be wholly inadequate by virtue of its failure to be a language at all, and thus has no advantages over the languages of appearances of the public kind, but does have insurmountable disadvantages. We know all we know from appearances, and we make, on the basis of these

[1] The Perceptual Element in Cognition, *Philosophy and Phenomenological Research*, 1952.

appearances, just whatever assumptions are necessary to make appearances consistent, and that is precisely the job of empirical science. We are all of us committed to some interpretation of the world in answering any question about the world; our choice is between the naïve-implicit or naïve-explicit assumptions of such as Broad and Price, or the explicit and sophisticated assumptions which are in keeping with the findings of modern science. What Price and Broad have usefully demonstrated for us is the inadequacy of Phenomenalism and the hopeless lameness of talking of the logical question of perception, when the questions asked are empirical. There are problems of language and problems of empirical fact and our job is to investigate the two together.

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VI.—NECESSARY TRUTH

By H. G. Alexander

The division of statements into two exclusive classes has been attempted by many philosophers. Leibniz divided them into necessary and contingent propositions, the former true solely by virtue of the principle of contradiction, the latter requiring also for their deduction the principle of sufficient reason. Hume distinguished relations of ideas and matters of fact. Kant, on the other hand, had two different and overlapping dichotomies, *a priori* and *a posteriori*, analytic and synthetic. Among recent philosophers some have held that the distinction is absolute and that what appeared to Kant as two different principles of division amount to the same thing—for them *synthetic a priori* is a contradiction in terms; others have tried to follow Kant and discover some synthetic necessary truths.

Of recent writers who have rejected 'Hume's Fork' as a comprehensive and exclusive dichotomy, two of the most interesting are Kneale and Quine. Both are writers who are chiefly concerned with the problems of science and both attack the usual formulation of this principle because it fails to give a satisfactory account of the nature of scientific laws. Kneale (*Probability and Induction*) points out that natural laws (and statements about the incompatibility of colours) are neither pure empirical generalizations nor mere verbal tautologies, and goes on to draw the conclusion that they are principles of natural necessitation. Quine ('Two Dogmas of Empiricism' in *From a Logical Point of View*, pp. 20-46, reprinted from *Philosophical Review* 1951) criticizes the distinction on the grounds that in science it is impossible to find any statements which are true solely in virtue of the way we use words. For him, it is nonsense to talk of a linguistic and a factual component in our knowledge.

In this article I shall attempt to discuss the value of these efforts to divide statements into two classes. In particular I shall try to sustain against Kneale the truth of Hume's contention that necessity exists in the mind not in objects; and against Quine that there is one sense in which one can distinguish a class of verbal tautologies from other statements.

I

One of the difficulties that confronts anyone wishing to discuss the problem is that of deciding which formulation of the distinction to consider first. Most writers have either concentrated on the definitions of ' analytic ' and ' synthetic ', or else have sought to explain what is meant in saying that the truth of some statements depends solely on the meaning of the constituent words. But a case can be made out for asking first what is meant by calling a statement necessary ? What is necessity ? And when we call a statement necessary does its necessity derive from some natural necessitation in the world, or from the way we formulate our knowledge ?

Kneale, strongly advocates the view that one can speak of ' natural necessitation '. Laws of nature are, for him, principles of natural necessitation. He reaches this position by arguments which are largely negative. There are, he suggests, four possible theories as to the nature of natural laws : (1) that they are propositions of restricted universality (Whitehead) ; (2) that they are not propositions at all but prescriptions (F. P. Ramsey) ; (3) that they state facts of unrestricted universality (Hume etc.) ; (4) that they are principles of natural necessitation—as, perhaps, implied by Locke in Book IV of the *Essay*. Kneale states cogently the objections to the first and second theories. He then argues that the third theory is also untenable. For if we have a natural law of the form ' All A things are B ' we can deduce that if this were an A thing it would also be B ; but one cannot derive an unfulfilled hypothetical proposition from a general proposition which is concerned only with the actual.

But this objection is not a valid one. The difficulty of deriving an unfulfilled hypothetical proposition from a proposition about the actual is only a form of the general difficulty of arguing from the observed to the unobserved—the problem of induction. If it is possible to infer from an examination of a finite number of things that all A things are B, then it is also possible to infer that if this were A it would be B.

The real objection to the ' constancy ' theory of natural laws is one that is implied in other parts of Kneale's book. It is that on this theory one counter-example would be sufficient to refute the law ; but that very few well-established natural laws would be abandoned by scientists in the face of one such counter-example. Confronted with this difficulty it is understandable that Kneale accepts the fourth theory that natural laws are principles of natural necessitation.

But can we say what 'necessity' means? Kneale writes:

> In fact, the word 'necessity' is the least troublesome of those with which we have to deal in this part of philosophy. For it has the same sense here as elsewhere. A principle of necessitation is a boundary of possibility, and we know quite well how possibility is bounded from consideration of such cases as the incompatibility of redness and greenness.

This seems over-optimistic. 'Possibility' is exactly as obscure a concept as 'necessity', and the incompatibility of redness and greenness is one of the most puzzling of all truths—if, in fact, it is a 'truth'.

I want to suggest as an alternative that rather than talking about natural necessitation, it would be more profitable to seek an elucidation of the concept of necessity by examining the different ways in which we come to abandon statements in the face of new evidence. If by considering how statements are refuted, one can explain necessity as a feature of the way in which we organize our knowledge, then one will be able to avoid Kneale's paradoxical theory as to the nature of scientific laws.

An obvious starting point for such an enquiry is Hume's examination of the idea of necessary connexion. Following him one might offer as a partial definition of necessity:

(*a*) a general statement is not necessary if one can conceive of one observable event (or instance) that would refute it.

Against such a criterion, Kneale brings the example of Goldbach's conjecture that every even number can be written as the sum of two primes. He writes:

> The attitude of mathematicians towards it can be expressed by the statement 'Goldbach's conjecture looks like a theorem, but it may conceivably be false' . . . . If the conceivability of the contradictory is not to be taken as a disproof of necessity in mathematics, where proof is attainable by *a priori* reasoning, why should it be supposed to furnish such a disproof in natural science? What we call conceiving is often little more in fact than readiness to manipulate symbols according to established rules.

To meet this objection, one must specify that by conceiving one means something like imagining, something like painting a picture of the contrary instance—and a picture with, if required, all the details filled in. Clearly in Kneale's sense one can conceive of physicists abandoning the Second Law of Thermodynamics,

but one cannot imagine any one specific experiment that would be interpreted as a conclusive refutation of the law.[1]

Now it is obvious that there are some generalizations which according to this criterion are not necessary. Consider 'No hawthorn attains a height of more than 60 feet', or 'In Britain, the dwarf juniper (*juniperus nana*) is found growing in places up to 2,700 feet above sea-level'. I now know that the second of these statements is false since I have found several dwarf juniper bushes growing at a height of more than 3,200 feet. I can conceive of the former being proved false equally conclusively.

There are, however, other important criticisms that could be made of this partial definition. One is that it makes it possible for a statement to be necessary for one man and not necessary for another. (More strictly, one man may be sure that it is not necessary, another man may not be sure.) Against this it may be argued that a statement must either be necessary or not necessary. But if one takes generalizations such as 'In all societies there is a different moral code for intra-group relations than for relations between members of the group and outsiders', it is quite possible that one writer may use this in such a way that it is open to refutation while another may not. (*Cf.* Walsh, *Proc. Aristotelian Society*, 1953-4.) Only in these parts of science that are not being actively developed, will there be a general uniformity of usage. It is also possible that any particular scientist may not have asked himself the question, 'What would I say if such and such an experiment produced such and such a result?', and that he might even refuse to answer the question if it were put to him by some inquisitive bystander. In short, one should be prepared to forego speaking of *the* necessity or *the* contingency of all statements in the same way that we refrain from speaking of *the* meaning of a word in cases where there are different usages.

So far we have only considered a definition which purports to state a sufficient condition for a statement not to be necessary. Can we go on to give a sufficient condition for a statement to be necessary? One is tempted to try this:

(*b*) a statement is necessary if one cannot conceive one observable event (or instance) that would refute it.

This definition, however, makes the necessity of a statement

[1] I have avoided discussing the question of what is meant by 'one experiment'. This can mean one particular experiment performed at a certain time in a certain laboratory; or it can mean a set of almost identical experiments perhaps performed in different laboratories. In discussing the physical sciences as opposed to largely observational sciences such as zoology, the second sense is the more important.

dependent on the imaginative powers of each individual, and could therefore lead to paradoxes. Thus it might be the case that some scientist would truthfully state that he could conceive of no one counter-example that would refute a hypothesis or law, but that when one of his colleagues produced an actual experiment with an unexpected result this would be admitted to count as a conclusive refutation.

A definition that would meet this difficulty is :

(*c*) a statement is necessary for X, if X is resolved that no one observable event would count as a conclusive refutation.

This definition would enable us not just to say that some statements *are* necessary (for X or for scientists in general) but that some statements *were* necessary but are so no longer. And there are cases in which this is just what we do want to say. Thus the statement ' The length of the sidereal day is constant ' could be said to have been necessary for Halley but not for the present Astronomer Royal. For, if pressed, Halley would have admitted that, since the sidereal day was the standard against which all subsidiary time-measuring instruments were calibrated, this statement was one which it would be impossible to refute. Or again we might say that the statement ' Every event has a cause ' was necessary for most philosophers in the past (with the possible exception of J. S. Mill).

Definition (*c*) is designed to emphasize that the necessity of a statement is not some essential property of the statement ; it is rather the way in which any man or any group of men uses the statement. This might be illustrated by a rough analogy.

The word *immovable* is defined in the *O.E.D.* as : (*a*) that cannot be moved physically. But one might perhaps define it thus ; (*b*) an object is immovable if one cannot conceive of any force that could move it. Definition (*b*) again appears to have the disadvantage that it would make immovability relative. But this is not entirely inconsistent with certain uses of the word. Sometimes we say ' Y was thought to be immovable before the invention of dynamite ' ; but sometimes instead we say ' Y was immovable until dynamite was invented ' or ' Y was immovable for the eighteenth century roadmakers '.

There is, however, another defect in this second definition. For it is possible that contrary to all expectations, the recalcitrant object might be moved by, for example, simple leverage. In such a case one would hesitate to say that it had been immovable, even though no one had conceived of it actually being moved. To avoid this difficulty one might instead say : (*c*) an object is

immovable for X, if X treats it as being such that no force could move it.

[This could be expressed by saying that there are two senses of *immovable*. *Immovable*$_1$ meaning ' incapable of being moved ' ; immovable$_2$ defined by saying ' Y is immovable$_2$ for X, if X regards Y as immovable$_1$'. One would then go on to admit that because of cobalt bombs, explosions of super-novae etc. nothing was in fact immovable$_1$. At this point the analogy with *necessary* breaks down.]

But rather than continue this attempt to analyse the concept of immovability, let me try to explain the sort of picture I have in mind here. Imagine some historic roadmaker—such as General Wade planning the route for a new road through the Scottish Highlands. One of his officers points out that at one point on the projected route there is a pile of massive boulders that have fallen from the rocky hillside above. After examining these, Wade and his assistants decide that they are immovable, and that it will be necessary for the road to make a detour to avoid them. The boulders cannot be altered and therefore it is the line of the road that must be changed.

In a similar way, if some statement such as ' every event has a cause ' is necessary for X, then whatever new evidence turns up, it is always some subsidiary hypothesis that is abandoned, never this principle. The principle, like the huge boulders, is taken to be fixed and unalterable.

Thus definitions (*a*) and (*c*) enable us to distinguish two classes of statement which I have called *necessary* and *not necessary*. Between these is the indefinite range of statements about which their assertors would be unwilling to commit themselves. But it is also obvious that in practice there are wide differences within the class of necessary statements. For—as has been pointed out by many writers on scientific method—it is very difficult in an advanced science such as physics to discover any laws which would be abandoned as a result of one experiment. But yet one would wish to distinguish between fundamental statements like the Second Law of Thermodynamics and other generalizations which could be abandoned without a wholesale reformulation of existing physical theory. In fact there is a strong case for introducing the apparently paradoxical notion of degrees of necessity. Degrees of necessity would then correspond to the degrees of tenacity with which the statements would be held in face of new evidence. And even the most fundamental ' laws ' have been abandoned. Both the geocentric theory and the principle that the sidereal day is constant, were finally rejected

as a result of observations. It became simpler to abandon them rather than to maintain them at the cost of excessive complexity in the resulting physical description of the world—just as, to revert to my analogy, the obstacles in the way of the roadmakers' diversion might have proved so great, that they instead went back to their original plans and, at great expense, moved the 'immovable'.

In this way one might attempt to group the laws of science in a hierarchy, the lowest level laws being those that could be altered comparatively easily and the highest level those that could only be abandoned at the cost of reformulating whole branches of theory. (*Cf.* S. E. Toulmin, *Philosophy of Science*, pp. 80-85.) But, of course, such a classification would be totally impossible in practice unless done retrospectively in a historical survey of science; it would be completely unprofitable for a physicist to spend his time in speculating about conceivable concatenations of unexpected phenomena.

II

The elucidation I have attempted to give of the two notions of necessity and non-necessity (or contingency) enables one to see some of the reasons that have led Quine and Kneale to their respective positions. For Quine, the fact that both the Second Law of Thermodynamics and the statement that every bachelor is unmarried, are necessary, leads him to deny that there are any statements which are vacuously true; it would be preposterous to say that it was only our language that made the Second Law of Thermodynamics true. Kneale, on the other hand, is struck by the impossibility of drawing any sharp line between scientific laws and principles such as 'Nothing can be both red and green all over at the same time'. The latter, he holds, are principles of natural necessitation known by intuitive induction and therefore all the laws of science are similarly principles of natural necessitation.

Kneale's line of argument is unsatisfactory, not because the reasoning is fallacious but because the conclusion which is reached turns out to be totally unexciting. To say that natural laws are principles of natural necessitation—and to say that probability statements are about degrees (or ranges) of possibility—is not an explanation at all. This is shown by the fact that many statements in science now taken to be false, *e.g.* 'The length of the sidereal day is constant' were once necessary truths. The most that Kneale can say is that some of the statements

of science which are now taken to be necessary truths, are genuine principles of natural necessitation, but that we can never be sure which these are. But this is almost the same as the early eighteenth century scientist who thought that in doing science he was discovering the workings of God in nature; he could never be sure that he had found the real laws but at any rate he knew that if he had, then they were laws imposed by God on His creation.

It is true that Kneale would reply that the phrase *natural necessitation* has a definite meaning in his work. But what is it? He cannot mean that all natural laws are known by 'intuitive induction' in the way in which we are said to know the incompatibility of certain simple colours—experimental science would then be unnecessary. All that he does tell us is that the view that natural laws are principles of natural necessitation is an alternative to Hume's theory that they state constant conjunctions and are thus refutable by one contrary instance. Certainly he is right in drawing attention to the inadequacy of the Humean account; but is there anything more to be said? If, as I have tried to suggest, it is possible to explain the appearance of necessity as a result of the way we systematize our knowledge and the way in which we alter and adjust this system to fit new observations, is there anything to be gained by calling this natural necessity? Just as in practice it makes almost no difference whether or not a scientist believes that in doing science he is discovering God's laws governing nature, so it is difficult to see that it makes any practical difference for a scientist to believe that he is discovering principles of natural necessitation—except in so far as it may make him regard the universe as even more mysterious than it seemed before.

III

So far I have attempted to discuss the concept of necessity with reference to refutation. From this point of view there may be differences in degree, but in general the class of necessary truths is a heterogeneous collection containing such diverse specimens as 'Every bachelor is unmarried', 'No real owl is stuffed', 'The wavelength of the red cadmium line is 6438 A°', and the First or Second Law of Thermodynamics. In particular it seems impossible to distinguish a sub-class of necessary truths consisting of verbal tautologies, that is, statements whose truth is entirely due to linguistic usage. This is substantially the argument used by Quine.

But if, on the other hand, one asks not how a statement might be refuted but how it has been established, then there emerges a very obvious difference between 'Every entomologist studies insects' and any accepted law of physics. For it is obvious that if anyone questioned the former he would be told 'That's just what the word "entomologist" means'; if he questioned the physical law, he would be told about a number of different experiments and would be shown how the interpretation of these led to the statement of the law.

The importance of this difference is emphasized by everyone who teaches introductory philosophy classes. One considers arguments such as this:

'Is it then the nature of a just man to harm any human being whatever?'

'Why certainly. He must harm those who are wicked and enemies.'

'When horses are harmed, do they become better or worse?'

'Worse'.

'Worse as horses, or worse as dogs?'

'Worse as horses.'

'And, similarly, when dogs are harmed, they become worse as dogs, and not worse as horses?'

'Necessarily.'

'Then, shall we not say the same of men, that when harmed they become worse as men, that is, worse in human excellence?'

Republic 335, Lindsay's translation.

It might seem to some simple-minded reader that this could be used as a conclusive argument against corporal punishment —until it is pointed out to him that the argument rests solely on one of the meanings of 'harm'. The practical question still emerges in the form 'Does corporal punishment harm?'

Another familiar example is the contention that all voluntary actions are selfish. Some people when confronted with this argument for the first time, are convinced by the apparently watertight reasoning involved and perturbed by the implications which it seems to have for morality. They are then referred to Butler's eleventh sermon and come to realize that precisely in so far as the statement is logically certain, it is practically unimportant.

These are standard examples, but although commonplace they are still useful. Plato was not the last man to treat tautologies as important truths about the world. But on Quine's argument,

this line of criticism is invalid; these two statements we have discussed may be as important as the basic laws of physics.

The nature of this difference can be seen more clearly if we consider the possibility of asking the man who says that all actions are selfish, to justify his assertion. In reply he will point out that all voluntary actions are ones which the agent wishes to perform, and that always to do what one wishes is what we mean by selfish. *A.* "But how do you know that all voluntary actions are ones which the agent wishes to perform? Have you made a survey of a large number of voluntary actions?" *B.* "Don't be obtuse. No survey is necessary. 'Voluntary' means 'doing what one wishes'; if a man did what he did not wish to do, the action would not be voluntary."

Quine would agree that the argument would take some such form. But he goes on to maintain, if I understand him correctly, that one can only justify the claim that 'voluntary' is synonymous with 'doing what one wishes' by doing the sort of empirical investigation that is usually left to the professional lexicographer.

Our argument must therefore go on thus. *A.* "How do you know that 'voluntary' means 'doing what one wishes'? Don't some people use 'voluntary' to mean 'not compelled'?" At this point it seems to me that *B* is very unlikely to allow the argument to be diverted into a lexicographical dispute. Instead he would probably say "There is definitely one sense of 'voluntary' as meaning 'doing what one wishes'. This is the sense in which I am using the word here." To use Robinson's terminology (R. Robinson *Definition*), *B* would refuse to argue as to whether he was giving the correct *lexical* definition (assuming for the moment that this is possible); instead he would admit that his definition was partly *stipulative*. He would in fact be getting near to the situation of which Quine writes (p. 26) "Here we have a really transparent case of synonymy created by definition; would that all species of synonymy were as intelligible. For the rest, definition rests on synonymy rather than explaining it."

Thus it is possible in a certain concrete situation to force a speaker to admit that the statement he is making is in fact a statement whose truth depends entirely on the way he is using certain words contained in that statement. In particular contexts then, it is possible to distinguish a sub-class of necessary statements which can be called analytic or tautologous. By an extension of this approach it does not seem unfair to say of, for example Plato, that, according to the most obvious interpretation, he is in such and such a place putting forward a tautology which he justifies solely on verbal grounds. In the actual example I

have quoted it is not unreasonable to say that if questioned Plato would have admitted that he was here using *βλάπτω* (to harm) in such a way that it meant making something worse.

To sum up my argument so far: I have agreed with Quine (and Kneale) that if one considers the way in which no one piece of new evidence would be taken to refute a necessary statement, then one will be unable to distinguish a special class of tautologies (or analytic statements). If, however, one instead asks what evidence is given (or implied) in support of an assertion, then, I have suggested, it will be possible to distinguish certain statements which are tautologies justified solely on the grounds that the speaker is using words in a certain way. But from whichever point of view the problem is approached, it must be remembered that one has to consider the statement in a particular context of assertion; the same form of words may be used by one man to make an empirical generalization, by another to utter a verbal tautology.

IV

There is, however, another way in which one might attempt to distinguish different types of necessary statement. This is by considering the different methods of defining the constituent words. As a first approximation one can distinguish five ways in which the meaning of words can be taught, three main ways and two subsidiary ones. (1) Verbal definitions, *e.g.* 'Triangle: three-sided rectilineal figure'. (2) Ostensive definitions, *e.g.* teaching the meaning of *midnight blue* by pointing to examples. (3) Definitions in use, *e.g.* teaching the meaning of *trick* by explaining the rules of one of the whist family of card games (*cf.* H. L. A. Hart, *Theory and Definition in Jurisprudence*). (4) Indirect ostensive definition, *e.g.* 'red: having or characterized by the colour which appears at the lowest or least refracted end of the visible spectrum and is familiar in nature as that of blood, fire, various flowers (such as the poppy) and ripe fruits (cherries)' (*O.E.D.*), or 'steppe: one of the vast comparatively level and treeless plains such as are found in S.E. Europe and Siberia' (*O.E.D.*). (5) Indirect definition in use, *e.g.* 'good: the most general adjective of commendation' (O.E.D.).

If one now takes Waismann's definition of *analytic* (*Analysis* 1949)—a statement is analytic if it can, by means of mere definitions, logical and further, linguistic (idiomatic) operators be turned into a truth of logic—it will be seen that the only statements which can be clearly shown to be analytic, are those in

which the important words can be verbally defined. For the notion of substituting definiens for *definiendum* makes no sense in the case of ostensive definitions or definitions in use.

This explains why the attempt to place all statements into one of the two pigeon-holes marked *verbal tautology* and *empirical statement* is specious when applied to much of our everyday discourse but ridiculous when extended to the physical sciences or to a deductive study such as economics. For much of our ordinary vocabulary does consist of words which can be defined either verbally or ostensively, that is words whose meaning can be learnt in isolation; and closely connected with this is the fact that many of our non-scientific statements can be confirmed or falsified individually. In science, on the other hand, few of the basic concepts can be defined by producing verbal equivalents. One can only teach someone the meaning of the word *temperature* as used in physics by teaching him the various ways in which temperature is measured over its whole range; and one cannot do this without explaining at some length the kinetic theory of gases and some thermodynamics. Or again the meaning of *force* in Newtonian physics can only be discovered by going through the classical theory of both statics and dynamics. Just as statements in physics 'face the tribunal of sense experience not individually but as a corporate body' (Quine, p. 41), so many of the words and phrases used in physics can only be understood by studying the corporate body of statements which forms that branch of physical theory.

It would, of course, be wrong to claim that in science there are no tautologies. 'If A and B occupy the same volume and A has twice the mass of B then the density of A is twice the density of B', would, I think, be regarded by everyone as a purely verbal statement. And it would also be wrong to say that outside science there are no 'necessary statements' which are not analytic. 'Nothing can be in two different places at the same time' or 'Things don't just disappear' have a status similar in many respects to that of scientific laws—which is one of the reasons why some writers have held that our material object language is analogous to a scientific theory. Words and phrases connected with space and time cannot (*cf.* Waismann) be defined either verbally or ostensively.

This threefold classification of definitions also goes some of the way towards explaining the peculiar puzzlement raised by statements about the incompatibility of simple colours, *e.g.* 'Nothing can be red and green all over at the same time'. This is unlike a statement such as 'All voluntary actions are selfish'

in as much as *red* and *green* unlike *voluntary* and *selfish* cannot be verbally defined; on the other hand it is unlike 'The length of the sidereal day is constant' (or 'All caesium atoms at all times have the same natural resonant frequency') in that it seems that one can teach the meanings of the words *red* and *green* individually by ostensive definition. One therefore does not wish to class statements about the incompatibility of colours with tautologies whose truth is due solely to linguistic usage; nor is it satisfactory to assimilate them to those 'necessary statements' whose necessity is a result of the way we systematize our knowledge. This is why one might be tempted with Kneale to class them separately as 'principles of modality', as cases where the necessity is not in us but in nature, even though one might go on to admit with Kneale (p. 35) that 'it is precisely in the ability to use words according to the rules of language that our knowledge of such principles ordinarily shows itself'.

But this threefold division is too neat. In particular, as Wittgenstein and Waismann have shown, the notion of ostensive definition is not nearly as simple as it appears at first sight. One can only understand an ostensive definition when one already has some idea of what it is that is being pointed out. Another difficulty is that it is easy to think of examples of words which are learnt in a way that, although it might be brought under the umbrella heading definition-in-use, is at the same time very different from the way we learn the meaning of words like *trick* or *temperature* or *force*. An important class of such words are the numerals, or the names of the days of the week. For we only learn to use the numeral '3' as one of a sequence of numerals; we only know how to use the name *Tuesday* when we also know how to use the names *Wednesday*, *Thursday*, etc.

These last two instances do, however, help to throw some light on the incompatibility of colours. For in these instances also we get 'necessary truths': 'If today is Tuesday, tomorrow is Wednesday', 'This stick cannot be both 3 feet and 4 feet long at the same time'. Such statements may not be analytic in Waismann's sense; but yet it seems unplausible to say that they are 'principles of modality'. Their necessity does not lie in nature. Now it also seems true to say that we learn the names of the simple colours as members of a group, and not in isolation. We can only learn how to use the word *red* by also learning how to use other colour words such as, perhaps, *blue*, *yellow*, *black*. *Red* does not mean *not-green*, as *barbarian* meant *non-Greek*; nor does *Tuesday* mean *not-Wednesday*. But we learn to use *red* and *Tuesday* respectively as members of a class of words which are

used as exclusive alternatives, the only difference being that in the latter case the alternatives form a definite cyclic sequence.

Thus, even if one admits that this division of types of definition is not completely satisfactory, the consideration of the various methods of learning the meaning of words does help to reinforce the view that there are no principles of natural necessitation. And also, since on any account of definition there will be a difference between verbal definitions and the rest, it will also be true to maintain against Quine that it does make sense to say that there are some statements whose truth depends solely on the way we use words.

V

From the analysis that I have attempted in this article, it is possible to draw two different conclusions. The more obvious one is that statements can be divided into three classes: verbal tautologies (statements whose truth is established by purely verbal considerations and which cannot be refuted by one counter-example), necessary statements which are not purely verbal (statements which cannot be disproved by one counter-example but which are established on largely empirical evidence), and contingent empirical statements. Or in a more familiar but less satisfactory terminology, analytic *a priori*, synthetic *a priori*, and synthetic *a posteriori* statements.

But it is perhaps better to conclude instead that the distinction between necessary and contingent statements or *a priori* and *a posteriori*, is so imprecise that the words that have traditionally been used to make the distinction are best avoided. For in the history of philosophy the writers who have used them most—with the exception of Kant—have been those who have denied that necessity and contingency depend on the way we formulate our knowledge. If then one wishes to emphasize that nothing is necessary independently of the way we express our knowledge and systematize it, and if one also wishes to maintain that statements can be 'necessary' at one time but not at another, then these phrases with their associations with either traditional metaphysics or with Kantianism, can hardly help being seriously misleading. To talk about grounds for asserting statements or possible evidence that would refute statements, may be slightly more cumbrous but this will be justified if one thus avoids some of the difficulties and misunderstandings that traditionally cling to discussions of *necessity*.

Note.—The discussion in this article applies primarily to general statements. But it could be extended comparatively easily to cover singular statements. The singular statements which can plausibly be regarded as necessary, fall into four groups.

(1) Those whose necessity is derived from the necessity of a corresponding general statement. For example, ' This piece of silver will melt at 961° C.' which is deduced from ' All silver melts at 961° C.'.

(2) Statements like ' Homer wrote the Iliad ' or ' Adam was the first man '. These, if necessary, are analytic in Kant's sense, since anyone who regarded them as necessary would be assuming that the *only* fact known about Homer was that he was the author of the Iliad, that is that we give the name ' Homer ' to the author of the Iliad.

(3) Statements which express an explicitly stipulated convention. For example, ' The metre bar in Paris (when maintained at a constant temperature of 0° C., etc.) is exactly 1 metre long ', or ' The wavelength of the red cadmium line is 6438·4696 angstroms (where the medium is dry air at 15° C., pressure is 760 mm. Hg and acceleration due to gravity 980·67 cm/sec.^{2}) '. The latter is now taken as stating the primary standard for the measurement of spectral wave lengths. It clearly presupposes the necessary truth of ' All specimens of pure cadmium under a certain set of identical circumstances exhibit identical spectra '.

(4) Statements about God. These pose a special problem which can partly be expressed by asking whether ' God ' is a proper name.

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VII.—DISCUSSIONS

CERTAINTY, NECESSITY AND ARISTOTLE'S SEA BATTLE

In her very original paper " Aristotle and the Sea Battle " (Mind, January 1956) Miss G. E. M. Anscombe argues in favour of a sense of ' necessary ' which is both non-logical and non-empirical. She claims for this the authority of Aristotle. I am not concerned here with the historical question of whether Aristotle in fact held the view which Miss Anscombe attributes to him, so that I shall say nothing about the exegesis of the *De Interpretatione*. Rather, I shall try to show that Miss Anscombe has failed to make out a philosophical case for the doctrine in question. I shall deal chiefly with a section (pp. 11-12) of the imaginary dialogue which is put forward as an elucidation of the Aristotelian thesis " from a modern point of view ", and a passage on page 14 where Miss Anscombe attempts to reconcile the opinions of Aristotle and Wittgenstein concerning the ' necessity ' of empirical propositions.

There are two theories defended in Miss Anscombe's paper, although she does not distinguish them. The first is as follows: Where p and q are different empirical propositions about the past and the present, the proposition $p \vee \sim q$ is a ' necessary ' proposition in a sense of ' necessary ' which is akin to, but different from, the sense in which $p \vee \sim p$ is necessary. The second theory is that p and q, defined as above, are individually necessary. Clearly, the first is a logical consequence of the second, but the reverse does not hold; that is, one can maintain that $p \vee \sim q$ is necessary without committing oneself to the view that p and q are separately necessary. " p and q are each necessary " entails " $p \vee \sim q$ "; but " $p \vee \sim q$ " does not entail " p and q are each necessary ". I shall consider only the more radical thesis, *viz.* the second. Miss Anscombe claims that " when p describes a past or present situation, then either p is necessarily true or $\sim p$ is necessarily true ".[1] In this sense, she continues, we can say that it is necessarily true that there is a University in Oxford. As Miss Anscombe points out, this sense of ' necessary ' is unfamiliar to us; we must therefore examine some of the remarks in the preceding dialogue which are designed to clarify it.

B. Are you necessarily here now ?

A. I don't understand you.

B. Could it turn out that this proposition that you, NN, are in All Souls today, May 7th 1954, is untrue ? Or is this certain ?

[1] *Loc. cit.* p. 12.

A. No, it is quite certain—My reason for saying so is that if you cared to suggest any test, which could turn out one way or the other, I can't see any reason to trust the test if, situated as I am, I have any doubt that I am here. I don't mean I can't imagine doubting it; but I can't imagine anything that would make it doubtful.

B. Then what is true about the present and the past is *necessarily* true?

A. Haven't you passed from certainty to truth? [1]

B. Do you mean to tell me that something can be certain without being true?—And isn't what is true about the present and the past quite necessary?

A. What does 'necessary' mean here, since it obviously doesn't mean that these are what we call necessary propositions?

B. I mean that nothing whatever could make what is certain untrue. Not: if it is true, it is necessary, but since it is certainly true it is necessary. Now if you can show me that anything about the future is so certain that nothing could falsify it, then (perhaps) I shall agree that it is necessarily true that that thing will happen.[2]

B's line of argument here is open to a number of objections.

(1) His thesis may be expressed in the form of the following syllogism:

All past and present empirical propositions are certainly true.
All necessary propositions are certainly true.
Therefore all past and present empirical propositions are necessary propositions.

This is a Second Figure syllogism of the form:

$$\begin{array}{l} S - M \\ P - M \\ \hline S - P \\ \hline \end{array}$$

But, of course, a syllogism of the Second Figure is invalid if, as here, the constituent propositions are *AAA*, for then the rule about the distribution of the middle term is broken. The argument can be rendered valid only by re-casting the minor premiss to read: All certainly true propositions are necessary propositions. This gives us a perfectly good First Figure syllogism in Barbara. But note that the new minor premiss is a much more questionable assertion than the original minor. Hence B and, it seems, Miss Anscombe, is either arguing invalidly from true premisses, or validly from premisses one of which is highly dubious.

(2) An independent criticism of this syllogism is that it commits

[1] A. should instead query the passage from certainty to necessity.
[2] *loc. cit.* p. 11.

the so-called fallacy of four terms, for as we shall see the expression ' certainly true ' is ambiguous.

(3) Even if this syllogistic version of the argument of the dialogue is not admitted as a fair account of it, the fact still remains that this discussion hinges upon a failure to clarify two different meanings of the expression ' certainly true '. There is no doubt that what A in the dialogue describes as " what we call necessary propositions ", *i.e.* tautologies, are ' certainly true '. And it is also beyond doubt that B is right in saying that very well established empirical propositions about the past and the present are also ' certainly true '. However, the two protagonists are using ' certainly true ' in quite different senses.

A logical truth like $p \vee \sim p$ is certainly true because, to quote, " nothing whatever could make what is certain untrue ". But note that nothing whatever could make a proposition that is true (in this sense) *true*. A tautology is true in itself, and cannot be " made " true or false, *i.e.* cannot be established or disestablished. In the sense in which the truth of a proposition is something that can be established, logically necessary propositions are neither true nor false.[1] Similarly, a logical truth cannot be doubted because there is nothing to doubt—for it asserts nothing. Thus it is nonsense, though on occasion it may be valuable nonsense, to ask: ' What makes a proposition like $p \vee \sim p$ true ? ' This proposition is necessarily true and hence certainly true ; but only because it is logically absurd to raise the question ' What makes it true ? ' The absurdity of this arises from the falsity of the assumption that something other than the proposition itself is the ground or guarantee of its truth. In the sense in which ' Is it true ? ' means, as it often does mean, ' What makes it true ? ', the question is here logically inappropriate. It is because a logically necessary proposition cannot be false that the concept of falsity (and *a fortiori* that of truth) is out of place.

This will become clearer if we examine what it means to say that an empirical proposition is ' certainly true '. This use of ' certainly true ' can be characterised as follows:

' p is certainly true ' is compatible with ' p is not necessarily true ', *i.e.* ' p is contingent '. In respect of an empirical proposition it always makes sense to ask ' What makes it true ? ', taking this to mean: What, other than the form of words which expresses the proposition itself, is its ground or guarantee ? If these grounds are capable of being, given, as they invariably must be if the proposition is really an empirical one and not a disguised tautology like ' I cannot remember another person's experiences ', then it follows that one can describe the sort of situation in which these grounds would *not* be given ; these would falsify, or at the least, fail to establish the proposition in question. I shall say more about this later.

[1] Note that in the dialogue B refers to the " truth " of $p \vee \sim p$ (p. 9). The occurrence of quotation marks here is important, although Miss Anscombe does not seem to recognise its implications.

A relatively minor difficulty is raised by B's challenge ' Show me a proposition about the future that is so certain that nothing could falsify it '. The phrase ' so certain ' is puzzling, for it suggests that there are degrees of certainty. This is quite admissible as a psychological notion, but as used by Miss Anscombe the phrase entails the logical absurdity that there are degrees of necessity.

As we have seen, Miss Anscombe defines her sense of ' necessary ' in terms of the concept ' certainly true '. The question therefore arises as to whether any propositions about the future can be said to possess this kind of necessity. In discussing this Miss Anscombe gives us an example of what she calls " an impossible prediction " (p. 13). Suppose that every day I receive a letter which gives an accurate account of all my actions and experiences from the time of posting to the time that I receive the letter. *A propos* of this, Miss Anscombe says: " Now, since we are dealing with what can be imagined and therefore can be supposed to happen, we must settle whether this would be knowledge of the future and whether its certainty would be a proof that what I did I did necessarily " (*ibid.*). The following paragraph shows why Miss Anscombe believes that the problem of freewill is relevant here. " It is interesting to note that Wittgenstein agrees with Aristotle about this problem, in the *Tractatus*. ' The freedom of the will consists in the fact that future actions cannot be known. The connexion of knowing and the known is that of logical necessity. " A knows that p " is senseless, if p is a tautology.' We are therefore presented with the logical necessariness of what is known's being true, together with the logical non-necessity of the kind of things that are known. The ' logical necessity ' of which he speaks in the remark on knowledge is thus not just truth-table necessariness. It is the unfamiliar necessariness of which Aristotle also speaks " (*ibid.*). The first point to notice here is that in holding that ' The freedom of the will consists in the fact that future actions cannot be known ', Wittgenstein is making a mistake. (We shall see that in defending this doctrine Miss Anscombe finds herself in serious difficulties.) If by ' know ' Wittgenstein means ' predict with a high degree of success ', which is all that ' know ' *can* mean with respect to the future, then he is in error, for in fact we never regard the predictability of a man's action as a reason for saying ' He couldn't help it ', *i.e.* ' His will was not free '. The question of whether a man was free in doing a certain thing is never answered by saying ' His action was accurately predicted beforehand '.[1]

Miss Anscombe is quite right in holding that, given ' A knows p ', it follows that ' p is true '. But this is simply a consequence of the logical grammar of ' know '; it has nothing to do with the alleged necessity of p considered in itself. Wittgenstein is of course right in saying that ' A knows that p ' is senseless if p is a tautology.

[1] Some arguments designed to establish this were put forward by the writer in ' Freewill : A Reply to Professor Campbell ', MIND, 1950.

However, one must notice that this proposition is also senseless if p is false—and in a different sense of 'senseless', for we are here excluding 'know' for different reasons. Because she has not made this distinction Miss Anscombe is led to endorse Wittgenstein's doctrine that a proposition which is known to be true is logically necessary in some special sense of 'logically necessary'. There is, however, no mystery here, for the meaning of 'know' is such that 'A knows p' and 'p is false' are contradictories. Furthermore, this holds quite independently of whether p is a prediction or not; that is, it has nothing to do with the question whether we could describe what would falsify p. As regards falsifiability propositions about the past or the present are not in any privileged position. In fact, with more plausibility, though still not without paradox, one could argue for the opposite view, *viz.* that predictions are by definition unfalsifiable, since they are testable only when they are no longer *predictions*.

In order to show that Wittgenstein's conception of freedom does not entail the absurd consequence that my actions as predicted in the hypothetical letter are not free, Miss Anscombe argues that even here, and presumably all the more so in ordinary contexts, we can never *know* what somebody will do. This is, of course, analytic, if 'know' is used in the sense in which 'A knows p' entails 'p is true'; but since we never use 'know' in this sense with regard to predictions, Miss Anscombe is forced to argue that there is another meaning of 'know' in which we never know the future. If 'certainty' = 'necessity' it follows that if we are certain of someone's future actions, these actions will be performed "necessarily". Another consequence of this, which Miss Anscombe does not mention, is that if we can be certain of what has been done in the past and the present, as we patently can be, then these actions are never free—yet it is precisely these actions which we praise and blame. I shall not discuss this, but the more central issue of whether it is correct to say that we 'know' p, when p is a proposition about the future. Miss Anscombe's remarks about this problem begin with a reference to the example of the letter. "Then this letter about my actions would not have been knowledge even if what it said was always right. However often and invariably it was verified, it would still not be certain, because the facts could go against it. But could the facts go against the sun's predicted rising? Is there not a radical disagreement between Wittgenstein and Aristotle here: Aristotle thinks that it is necessity [1] that the sun will rise, Wittgenstein says that we do not know that the sun will rise; and that the events of the future cannot be inferred logically from those of the present. But he also says that we could not say of a world not going according to law how it would look. So though he thinks that anything describable can happen, he would enquire whether the sun's rising tomorrow is a describable event. So why does he say that we do

[1] I presume that this is a misprint for 'necessary'.

not know that the sun will rise? Not, I think, because the facts may falsify the prediction, but because there may not be any more facts; as in death the world does not change but stops." [1]

I now propose to consider a number of difficulties that are raised by this passage.

(1) When Miss Anscombe says that however often the predictions in the letter were verified they would still not be certain "because the facts could go against them", she is giving to 'certain' a sense which is unusual in this context—for we normally say that we are certain of many propositions which, in some sense of 'could', could be falsified by the facts. These, in fact, are just the sort of propositions in respect of which the word 'certain' is used. Miss Anscombe does not seem to have grasped the point of Wittgenstein's remark, quoted on page 13: "'A knows that p' is senseless if p is a tautology." From this it follows, quite correctly, that if p is synthetic 'A knows that p' is not senseless. Factual predictions (*i.e.* predictions in the proper sense of the word) are synthetic, and hence we can say that we are certain of a prediction without raising the question, which Miss Anscombe apparently regards as separate from the 'analytic *v.* synthetic' distinction, whether "the facts could go against it". In any event, it is by no means clear what it means to say that the facts *could* go against a certain proposition. If this 'could' refers only to logical possibility, then the remark is a triviality, following from the definition of 'prediction', which is simply a type of empirical proposition. On the other hand, if we interpret the 'could' as referring to an empirical possibility, then it is quite incompatible with the meaning of 'empirical' to say that an empirical proposition cannot be falsified by the facts, whether the proposition deals with past, present or future facts.

(2) What is one to make of the question: Could the facts go against the prediction that the sun will rise tomorrow? Miss Anscombe treats this question, I think rightly, as equivalent to: what are the facts which would falsify this proposition? There is here, she says, an apparent "radical disagreement" between Aristotle and Wittgenstein. Aristotle thinks that it is necessary that the sun will rise, Wittgenstein that we do not know if it will rise; he says, with Miss Anscombe's approval, "the events of the future cannot be inferred logically from those of the present". We must now ask: what is the force of 'logically' here? Let us suppose that the word is redundant, so that the sentence should read: 'The events of the future cannot be inferred from those of the present.' This is clearly false, for we are continually doing precisely this, and with a high degree of success. But suppose that 'logically' is not redundant; in that case what we are being told is that factual propositions about the present do not entail factual propositions about the future. This amounts to a truism that no one would wish to dispute, namely that a prediction is not a logical truth. It

[1] *Loc. cit.* p. 14.

does not, however, show that there is some special sense of ' necessary ' in which empirical propositions are necessary.

(3) Another consequence of this is that logical truths are not predictions. To say ' Either the sun will rise tomorrow or the sun will not rise tomorrow ' is to make no sort of forecast. The temptation to suppose that this is a genuine prediction arises only from the *grammatical* similarity between this form of words and sentences like ' Either you will take this medicine or you will not get better '. I can test this piece of advice, and my acceptance of it will modify my behaviour. But if I were to agree with (and I cannot fail to do so, which shows that there is here a misuse of the word ' agree '), ' Either you will take this medicine or you will not take this medicine ', then nothing whatever follows.

(4) Miss Anscombe endorses Wittgenstein's opinion that the sun's not rising tomorrow is indescribable. Neither Miss Anscombe nor Wittgenstein, I think, would dispute that the sun's rising tomorrow *is* describable. I would maintain that from this it follows that the sun's *not* rising must also be describable. If we know the sort of circumstances in which we can truly say ' The sun is rising ', this entails that we also know, and can describe, if only by negations, the sort of circumstances in which we can truly say ' The sun is not rising '. It should be pointed out that this consideration is in no way affected if the example in question is a prediction like ' The sun will rise tomorrow '. Admittedly, what verifies or falsifies this will be something that will happen tomorrow ; but if I cannot describe what is conveyed by ' The sun rose (or did not rise) this morning ', then I can give no meaning to the prediction ' The sun will rise tomorrow '.

(5) It is not easy to see how Miss Anscombe proposes to reconcile the differences between Aristotle and Wittgenstein about the status of the proposition ' The sun will rise tomorrow '. Indeed, the problem as here stated is, to me, far from clear. Miss Anscombe ascribes to Wittgenstein the view that we do not know that the sun will rise tomorrow ; she wishes to show that this is compatible with Aristotle's doctrine that the same proposition is necessary. On Wittgenstein's theory, it is alleged, there is a doubt whether the contradictory of the proposition is verifiable, and therefore, for reasons that I have given, that the proposition itself is verifiable. Hence, so Miss Anscombe seems to imply, Wittgenstein would be inclined to agree with Aristotle in holding that it is a necessary proposition, in some sense of ' necessary '. Clearly this cannot be, as Miss Anscombe claims, the sense of ' necessary ' which she has been discussing, for that has heretofore meant ' certainly true '—and Wittgenstein is *denying* that ' The sun will rise tomorrow ' is certainly true. Hence it appears that Miss Anscombe can claim that there is agreement between Wittgenstein and Aristotle only by assigning to ' necessary ' yet another, and unspecified, meaning.

(6) Miss Anscombe believes that Wittgenstein's remark " We could

not say of a world not going according to law how it would look" is relevant to the question of the alleged impossibility of describing the sun's not rising tomorrow. I think there is a mistake here. Wittgenstein's statement is surely intended to be taken quite generally, as referring to the regular behaviour of the world as a whole. If so, then the question of whether a *particular* law or group of laws, even important astronomical laws, will continue to hold, is a quite separate issue. According to Miss Anscombe, Wittgenstein's view leads to the paradoxical and implausible consequence that if the sun did not rise tomorrow, we could not describe *any* features of the world. This is absurd, because for all we know nature might continue her regular courses in every other respect. Only if a high proportion of fundamental physical laws ceased to apply would the events in question be indescribable.

(7) A subsidiary problem that now arises is this: is not Wittgenstein's statement that if the sun does not rise tomorrow "there may not be any more facts" *either* a prediction in itself, or, if we stress the 'may', an evasion of the problem?

(8) In attempting to explain this Miss Anscombe cites an analogy that is not helpful—"as in death the world does not change but stops". In the only sense in which I can understand it this statement is clearly untrue. The death of a particular person or persons does not "stop the world"; life, as they say, goes on. I suspect that Miss Anscombe is inclined to believe this falsehood because she is here following Wittgenstein in his solipsistic vein, in confusing 'the world' with 'my world', *i.e.* 'my experience'. The proposition 'With death my experience stops' is *a priori* true, if these terms are appropriately defined. But of course this does not dispose of, or even touch, the question as to whether I shall have any experiences after I am (clinically) dead. We shall see that there is another reason why Miss Anscombe thinks that there is a connexion between the notion of death and the meaning of the proposition 'The sun will rise tomorrow'.

(9) The unfamiliar sense of 'necessary' which Miss Anscombe is introducing, or rather for which she is claiming Aristotle's authority, strikes one as very strange if it is taken to apply, and there is no qualification to suggest that it does not, to *all* propositions about the past and the present. Here it is important to notice a peculiarity about some of the examples which Miss Anscombe gives of "necessary" empirical propositions. These are: (*a*) 'The sun will rise tomorrow', and (*b*) 'There is a University in Oxford'. (There is a third, false, proposition which is given as an example, about a civil war that raged in England between 1850 and 1870, to which these remarks do not apply.) I wish to argue that the peculiarity of propositions (*a*) and (*b*) derives from the fact that it is easy to think of them as tautologies, though concealed ones. For this reason it seems plausible to Miss Anscombe to say that they are necessary, but—because their tautological character is not explicit—necessary

in some special sense of the word. Let us consider these two propositions separately.

(*a*) This is clearly tautologous if 'tomorrow' is defined in terms of the next sunrise. This definition of 'tomorrow' is quite in conformity with ordinary usage, for we normally mean by 'tomorrow' the period which begins with daybreak, *i.e.* the sunrise. Thus we can say that it is necessary that the sun will rise tomorrow in precisely the same sense that we can say that it is necessary that the winner of the race will be the first past the post—and this is not tipster's advice, or indeed advice at all. If the sun did not rise there could not, logically, *be* a tomorrow, so that (*a*) is simply asserting the analytic connexion between the protasis and apodosis of the hypothetical 'If there is a tomorrow, the sun will rise'. This provides a possible explanation of Miss Anscombe's statement that if the sun does not rise tomorrow "there may not be any more facts". If I am right in suggesting that she tacitly regards (*a*) as a tautology, then it is not difficult to see the connexion between its contradictory and the somewhat obscure idea that in those circumstances "there may not be any more facts". If it is logically impossible for there to be a tomorrow, then there can be no future; and if there is no future, no knowledge in the future, and hence no facts to know. I suggest that it is for this reason, which we have seen can be shown to be a matter of definition, that Miss Anscombe mistakenly supposes that the concept of death is relevant here.

(*b*) Properly speaking, no proposition of the form '*X* is *Y*', where '*X*' is a proper name, can be described as a tautology, for the simple reason that a proper name is not a descriptive phrase and hence has no definition in the strict sense. Nevertheless, there is usually a generally accepted denotation for a given proper name, so that if this is explicitly stated in the predicate of a proposition like '*X* is *Y*', the whole may be regarded as an analytic proposition.

The proper name 'Oxford' is generally taken to refer to a city which contains a University; indeed, the name 'Oxford' is more often used to refer to the University within the city than to the city itself. Consider another example. The name 'Roger Bannister' is conventionally taken to refer to an athlete; thus the proposition 'Roger Bannister is an athlete' is, in a sense, true by definition, *viz.* the conventional significance attached to the name 'Roger Bannister'. The equally true proposition 'Roger Bannister is a doctor' is, ordinarily speaking, synthetic, because this proper name is not normally taken to be the name of a doctor.

Hence if it is true, as I think it palpably is, that the name 'Oxford' is as often used to refer to a University as it is to a city in the South Midlands, then it follows that it is (logically) necessary that there is a University in Oxford. In other words: 'Without the University Oxford would not be Oxford'. Some people lament that Oxford is not the same in the Long Vacation; they are asserting, I think rightly, that there is a necessary (*i.e.* logical) connexion between the

concepts of 'Oxford' and 'University'. It is part of the meaning of the name 'Oxford' that it should refer to a University, and so a matter of definition that there should be a University in Oxford. One does not say that there is a University in Oxford in the same sense that one says (truly) that there is a statue of Queen Victoria in Reading. The statue can be transplanted without affecting the meaning of the expressions 'the statue of Queen Victoria' or 'Reading'. But suppose that the dons, administrators, and undergraduates of Oxford were evacuated to Middlesbrough—would we then say that this is still Oxford University, as the name is ordinarily understood? And would the name 'the city of Oxford' retain its old meaning?

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PROFESSOR SHWAYDER ON " = "

MR. SHWAYDER'S rewarding article, " = " (MIND, vol. lxv, No. 257, January, 1956), points out that the use of identity-like terms may result in ambiguity and a certain awkwardness of expression. After considering the problems in some detail he proposes a " fundamental criterion of identity ". However, his treatment demands further consideration. Rather than attempt a summary of the article, I will remind the reader of pertinent sections in connexion with the points I wish to make.

(1) The statements introducing the article are puzzling only because the distinction between the etymologies of identity-like terms and a proposed concept of identity was ignored. (2) The distinction between descriptive and identity statements is invalid. (3) The problem of identity does not constitute a philosophical puzzle.

I

Shwayder begins by asking the following questions (my divisions for purposes of identification): (m) " What is intended when we assert that *two* things are numerically identical, (o) are equal or (x) are the same *one* thing; or (o) when we say that two things are the same in some one or other respect? (m) What is the characteristic force of " = " as used in such expressions as,

$$\text{" } x^2 - 4 = (x - 2)(x + 2)\text{"? "}$$

Also, (c) " What kind of property could it possibly be that we predicate of an object when we say that it is the same as another object, or, even (x) the same as itself? " " . . . (o) What precisely the difference is in saying that two things are alike in some one or several respects and (x) in saying that two things are really one, (x) are identically the same.

The sign appearing in the title of the article appears only once in this admittedly puzzling group of questions. Others are: " identical ", " equal ", " one thing ", " the same ", " the same as ", " alike ", " really one ", " identically the same ", " the same as (itself) ". In this discussion they are spoken of as identity-like terms. If it is assumed that the statements mentioned and referred to are statements in use, then the puzzle is solved when they are unscrambled and the universes of discourse and/or relevant languages in which they originally occurred are named. From the cues available, I would solve it in the following manner. The " m " statements appear in mathematical systems or translations of mathematical systems into ordinary languages where the uses are governed by explicit rules and the " intent " is clearly known. The " o " statements occur in ordinary language as descriptive complex sentences and may or may not bear some analogy with formal or

mathematical uses. The " c " statement seems to be a rough translation into ordinary language of a formal proposition in class calculus, otherwise it may be considered an " o " statement. The redundancy in the " x " statements suggests a rhetorical context although the expression, " the same as itself ", is troublesome. I am guessing that it was an attempt to translate the " A = A " in traditional logic in such a way as to discourage further discussion.

Now it is obvious that the puzzle was not carefully constructed for there seems to be more than one answer. Whereas I find several contextual meanings for the identity-like terms and no problems of usage within the respective contexts once identified, Shwayder finds only one meaning and several problems. He states that " the meaning of equations varies as between different domains of discourse ", but he also states (as a fact) that " the meaning of " = " remains the same " (p. 27). What is meant here is not clear. Strictly speaking, " = " does not occur in ordinary language but the other terms listed above do. If it is maintained that all of these terms are synonymous, something which seems to be assumed but is never made explicit and could only be argued in the most general terms, then he has yet to show that there is any sense in which one provides the explication for the others. Of course it is analytic that " the meaning . . . remains the same ", but assuming that the claim is to be interpreted factually, we only have recourse to the various etymologies of the terms introduced. If it were found that every time a given term were used a given meaning could be shown to correspond to it and then if it were found that these meanings, independently arrived at, possessed some common feature, the discovery, as with any historical discovery could only be stated in the past tense. Thus to preserve its factual character the statement, " the meaning of ' = ' remains the same ", must be rewritten in a metalanguage about all other languages, " the meaning of ' = ' has remained the same ".

Just how this meaning of the identity-like terms, apart from the sentences in which they have occurred, is to be determined is difficult to answer. For the present, however, we will leave that to the etymologist.

If it were found that these various usages all possessed some specifiable characteristics in common it might be argued that we possess a concept. They could be said to be roughly synonymous and *each* would become a concept-word. Using Frege's distinction between concept and object, we could speak of the establishment of *an* identity as compared to *the* identity, say between A and B.[1]

But do we possess a concept of identity which permits us to speak unambiguously with regard to it in any domain of discourse? Obviously not, otherwise the introductory questions quoted above would

[1] Gottlob Frege, " On Concept and Object ", reprinted in *Translations from the Philosophical Writings of Gottlob Frege*, ed. and trans. by P. Geach and M. Black, Oxford, 1952, pp. 42-55.

have constituted no problem whatsoever. At most, we possess concepts within domains of discourse. The fact that the most cursory etymologies are notoriously difficult to obtain and are seldom up-to-date does not grant us licence to commission concepts, particularly in a natural language. Nor does the recognition and acceptance of this limitation subject us to the charge that we are psychologizing formal concepts. Within formal systems *the* meaning is rigorously stipulated.

II

A sizeable portion of Shwayder's article is concerned with showing that " identity statements " must be distinguished from descriptive, existential and universal statements. " My main contention is, therefore, that the use of ' = ' marks a fact-stating use of language which is characteristically different from those uses of language which are exemplified in such statements as ' The moon is blue ', ' There is a blue moon ' and ' All moons are blue ' " (p. 23). Again, " I think that almost all the puzzles adumbrated above and, indeed, that almost all the proferred solutions derive ultimately from the covert but erroneous assumption that equations are, if meaningful, necessarily used to describe something or other " (p. 23). Not only are identity statements distinguishable from descriptive statements, but they are " indispensable in almost all domains of discourse " (p. 23). " Judgements of identity and existence are, as it were, on a par with describing ; are not varieties or species of describing " (p. 24).

The status of universal and existential statements will not be discussed here as they demand separate treatment. It goes without saying that all formal systems which introduce " = " or employ it as a primitive term have it as a constituent element of the system. But it is not true that (*a*) all formal systems employ the sign, or (*b*) that, whether formal or natural, those which do are unable to translate identity statements into other forms, or (*c*) that future formal systems must include the sign as a constituent element.

Once again, we are forced to consider uses within universes of discourse. In semantic languages, save for definitions, identity-like terms usually occur in statements which are incomplete as to their meaning and may be written as two or more objects having the same properties. Such terms simply serve as shorthand for the detailed list of properties. Like other shorthand terms, far from being indispensable, their use frequently results in considerable confusion. This provides a clue as to why identity statements are customarily considered as instances of description. They imply one or more standpoints, common properties, which if the meaning is to be made clear must be mentioned in the context (Wittgenstein, *Tractatus*, 6.2323). When the number of relevant properties being considered is small, the identity statement may appear as an independent clause where the properties are mentioned in the remainder of the sentence (*cf.* the " o " statements above). It is a question of style as to

whether or not one wishes to rewrite it as a simple sentence without the identity-like term. However, when the number is large they may appear as complete sentences where the context serves to complete the meaning. Formal systems stipulate axiomatically that property or relation by which the expressions are identified. If within the system a problem of identity or for that matter any other problem arises, this can only be interpreted as one of the limits of that particular system. There is certainly no justification for questioning the use of similar notations in other systems.

It is the fact that they may appear as complete sentences both in semantic languages and in formal languages that permits Shwayder to distinguish them from descriptive statements. His analysis of the concept of identity shows it to be made up of two seemingly overlapping notions, " location " and " primary location ". Following Strawson's usage (" On Referring ", MIND, 1950, pp. 320-344) Shwayder states that significant equations may be formulated only with the use of " referring expressions ". " In holding that expressions which may be significantly adjoined to ' = ' are referring expressions, I imply, among other things, that they are not predicates or relations " (p. 30). There must be, not only a referent, but a unique referent. Referring expressions enable us to " locate " the particular object, *i.e.* using Shwayder's example " '2^3' may be said to determine the location of its referent as the number we get by multiplying 2 by itself three times " (p. 31). (Whether or not in another context " 8 " may be thought of as a referring expression and " 2^3 " as the referent, the object, is not clear, or for that matter, " (2×4) ", " $\sqrt{64}$ ", etc.) Certain expressions determine the " primary location of an object ". The best examples are proper names, " we might say that the object *is* its primary location " (p. 31). The notion of " primary location " opens the way for showing the relation between the " various criteria we may use in order to establish identities ". This relation is called the " fundamental criterion of identity ". " We determine the fundamental criterion of identity merely by specifying the primary locations of objects. This means that for every expression ' E ', *if* ' E ' determines the primary location of an object, E, then ' E = E '." Such identities are said to be trivial, but non-trivial identities (*e.g.* " A = B ") are proven when they are reduced to trivial identities (p. 33).

Thus identity statements are unique in that while descriptive statements may include referring expressions and must include expressions denoting properties or relations, identity statements include only referring expressions. Shwayder treads a very thin line indeed when he states that equations may serve a fact-stating function, *i.e.* be other than trivially true, and at the same time include no descriptive terms. This is the burden of his article but it is invalid inasmuch as such statements are incomplete. Otherwise they are trivially true in the sense that the context, though patently shared, is unexpressed. Apart from some context in which

a property or relation is mentioned or implied, strictly speaking, identity statements are unverifiable.

The fallacy of Shwayder's reasoning becomes clear when it is suggested that what he means by an identity statement is what may be described as a "referring statement" and that this is to be distinguished from a descriptive statement. The distinction between referring expressions and descriptive expressions (Strawson) becomes a distinction between statements. Furthermore, this is evident in the extent to which he went to find examples for identity statements which were more than trivially true and were non-descriptive. (*E.g.* the "problems" involved in "$e^{\pi i} = -1$" have nothing to do with the use of "=", pp. 32-33.) It should also be clear now why it was first necessary to employ "=" as a concept term essential to every language but semantically transcendent and distinguish it from properties and relations. Only in this way could identity statements be declared different in kind from descriptive statements and justify a unique criterion.

It is indeed curious that such treatments frequently conclude by explicating *the* meaning of "=" with statements of the form, "A = A". From the viewpoint of usage it is this form which requires explication, it occurs only under the most extraordinary circumstances in the most varied contexts (*e.g.* in poetry, as a "stop sign" in formal proofs, as a "law" in some formal systems, etc.). There was some practical convenience in proclaiming it a "law of thought" so long as it was never interpreted or applied. To say that it is analytic does not clarify its usage, since that respect in which it differs from other analytic statements is just that which requires clarification. Are there degrees of analyticity? To interpret it semantically by reading "itself" for one "A" not only introduces a problem of interpretation (*i.e.* why not read "itself" for the other?) but begs the question of significance. We do not ordinarily say, "This is identical with itself", although our language permits it. Even here, we are begging the question by supposing that there is *a* form of the statement such that it can be translated from one realm of discourse to another.

Wittgenstein (*Tractatus*, 5.5302) commits it to a convenient limbo by defending its significance, *i.e.* it is theoretically possible that two objects may have all of their properties in common. There are immediate objections to this but no more than those which attend the judgement that it is trivially true. Wittgenstein's style grants him precedence here for it is obvious that whatever may be said cryptically about it would have a distinct advantage over an extended treatment.

III

The practice of speaking of such treatments as puzzle-solving requires some comment. Supposedly, something is asserted when it is claimed that questions such as this may be considered as puzzles.

If so, it is false. A puzzle for which there are many possible answers is a contradiction in terms, furthermore, for most puzzles we not only accept the answer in principle but we know what the answer is before beginning. Puzzles imply a puzzle-maker, if there are " natural " puzzles then they must imply a Puzzle-Maker. We are entertained when the limiting conditions are obvious, the cues are about equally suggestive and when a method usually unique to that puzzle gradually evolves which leads to the solution we have already accepted as *the* solution.

These conditions do not obtain in treatments of this type. Before being solved, philosophical puzzles would first have to be constructed. Formal proofs bear at least a rough analogy to the ordinary meaning of " puzzle " and quite appropriately may be spoken of as philosophical puzzles. But this is true only if the proof has already been formulated and the steps are unknown to the puzzler.

Perhaps the usage is intended to suggest that we find some given problem puzzling. If so, it may be construed not as a statement about the problem but as a statement about our own behaviour in regard to it. Not all problems we find puzzling are in fact puzzles. If it is intended to suggest that such problems are entertaining and/or trivial, then the judgement ceases to be descriptive and becomes evaluative.

Shwayder could perhaps argue that in this case he is the puzzle-maker and that he was concerned with its formulation, not its solution. But of course this would have called for a somewhat different treatment. The placing of scrambled elements in relevant categories is a common technique in puzzle-solving and if this was intended, then an attempt was made to solve it in Part I of this discussion.

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A NOTE ON SAVERY'S EMOTIVE THEORY OF TRUTH

(MIND, vol. xxiv, No. 256, October 1955)

I AM not at all sure that I understand the naturalistic fallacy in the ethical context: I am sure that I cannot understand it in the "truth" context. What would it be not to define "truth" in terms other than truth terms—what would be the point of defining it *in* truth terms?

I am not certain that definitions of truth are a good idea, which does not mean that I think that we can altogether avoid them. What is quite clearly *not* a good idea is (1) the restrictive definition of truth which eliminates certain sets of criteria, of which an example would be the alleged Patristic sort of definition, (2) the "philosophical" view that, once a criterion has been selected and so excluded other criteria, then *truth* can be defined solely in terms of this criterion, for all contexts whatever.

If I say "I know the truth about the Brown, Jones affair", I can then be made to commit myself to something about the borrowed £10 and the black eye in the public bar, and to back up what I say with the evidence of eyewitnesses, letters, etc. "Having the truth about it" is extremely difficult to define, but if I say "I *have* the truth about the Brown, Jones affair", it is fairly clear what I mean, and can be made, progressively, very clear indeed.

When I say, with an early Father perhaps, "I have the truth about the Trinity; there are three persons in one God", it is then appropriate to talk about "faith" rather than 'What I have as evidence' in the way I have evidence in the Jones, Brown case. For the way in which I decide whether certain remarks about the Trinity are true or not is not the way in which I decide about people's claims to know other sorts of things. In the theological case my criterion may very well be "Faith" whatever that may mean. "Faith" of course must be analysed. Some people, myself included, would not be at all happy about remarks like "if faith disagrees with reason, so much the worse for faith". This, however, is a matter for possible conflicts of criteria, and not directly to the point of this note.

In the two sorts of cases, the Jones, Brown affair and the Trinity, the matter is different. The ways we check up are different. If this then means that we cannot use "true" in the same sense in both, then it means we cannot use "true" in the same sense in both; but does this surprise anyone? What would it be to use "true" in the same sense? What is it that we cannot do when we cannot use "true" in the same sense? What we cannot do is to say both that:

1. "'True' is defined tautologically in terms of X check up procedure."

"'True' is defined tautologically in terms of Y check up procedure."

"X check up procedure is different in type, etc., from Y check up procedure."

and:

2. "'True' has the same meaning in both cases."

What "'True' means the same" might very sensibly mean is: "'true' is used when each proposition has been checked up with 'the subject matter', 'the facts', 'the world' by the appropriate procedure, and been found to 'fit the facts, etc.'" This seems obvious, perhaps trivial, but extremely important.

. . . . . .

Mr. Savery says (p. 520):

> The interesting question perhaps is: what basis do we have for accepting one criterion of truth rather than another? It will not help to assert that knowledge is the basis, because this either begs the question or is circular. It should be clear that our basis for accepting one criterion for truth rather than another depends on some will-attitude, some interest, some arbitrary fiat, something that is describable in non-rational rather than rational terms.

The fact is, of course, we have a basis for selecting a criterion when we select it. I know what is meant by saying that 'Verifiability' is the appropriate criterion for the truth of X, when X is something of the sort "This is sulphur", "This stuff explodes". We know what is meant by saying "The party says so" is not an appropriate criterion for the truth of Y when Y is "heredity is less important than environment" "Jesus was not a Jew" and the like. But *how* do we know?

We may very well "know pragmatically", but I do not think that this suggestion makes me any of the sorts of pragmatists that Mr. Savery lists. It is difficult to account for how we know, or what we know, when we know "it is an appropriate criterion", but this does not make the matter non-rational. I *understand* the appropriateness perfectly well, and could *defend* it: even if I found the "philosophical" construction of a set of "canons of appropriateness" difficult or futile.

What is interesting is that a criterion of truth settled on in advance, let us say by a 'will-attitude' (p. 251), might be inappropriate, and could be shown to be. If I want to get a man to stop using an inappropriate criterion it may be difficult, but it is not either factually or logically impossible. It is not circular to say "this criterion X of *truth* that I am using here is, (and yours is not) the *true* one" if all I am doing is, for example, showing a man that "the party says so" is *not good grounds*, sufficient evidence, etc., for saying "and

so it *is* so". We can show a man that it is not good grounds in one of at least two ways:

1. By showing, on the contrary, what *would* be good grounds, etc.
2. By showing him, that if that is all that he *means* by "true" then: "true" = "What the party says". But surely this is very unlike the ordinary sense of "true" in which the tale told to you by the man in the next door flat about the state of Thompkins' dust-bin was "*true*".

We should probably play out both of these in an attempt to show a man how his criterion was inappropriate. We should not be showing him nothing.

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USES OF ANALYTIC STATEMENTS IN ORDINARY DISCOURSE

In a suggestive paper in the July 1956 issue of Mind, Mr. D. W. Hamlyn proposes the following theses:

1. What have traditionally been called "analytic statements" have no use in ordinary language (p. 361).

2. If anyone were to utter a sentence which might be claimed as expressing an analytic statement, he would not, in fact, be aware of the implications of his own use of words (p. 360).

3. The term "analytic" is employed as a term of logical appraisal, not for the allotting of statements to one class rather than another (p. 359).

Mr. Hamlyn's arguments in support of these theses are lengthy, and since his article is readily available, I shall not recapitulate them. However, I should like to show in this note that all of the above theses, plausible though they may seem, are mistaken. This can be done as follows:

1. The first thesis can be confuted by producing an example of an analytic statement which has a use in ordinary language. I refer Mr. Hamlyn to Act I, Scene V of *Hamlet*, lines 123-124, where Hamlet says to Horatio: "There's ne'er a villain dwelling in all Denmark, But he's an arrant knave." This is an example of an analytic statement (it will be remembered that even Horatio says, "There needs no ghost, my lord, come from the grave, To tell us this . . ."), and it has a describable use. Hamlet wishes to fend off Horatio's questions about his conversation with the ghost, and he does so by uttering a tautology. He is thus able to seem to answer Horatio's persistent questioning without revealing any of the information given him by the ghost. This example is similar to sentences in speeches made by politicians and for the same purpose, *i.e.* to allow the speaker to seem to be conveying information even though he is in fact not doing so. Every parent who has dealt with a recalcitrant child will also recognize this use of language. Tautologies may be employed, in short, in order to avoid committing oneself to anything. Mr. Hamlyn's mistake is to infer that because analytic sentences convey no information they have no use in ordinary language, whereas we are suggesting that some utterances have a use in ordinary discourse just because they do convey no information.

2. Since Hamlet obviously realized the implication of his use of words (his answer to Horatio's rebuke that he has said nothing is, "Why, right; you are i' the right"), Mr. Hamlyn's second thesis, *e.g.* that someone who employs an analytic statement would not in fact be aware of the implication of his own use of words, is clearly false.

3. The third thesis is mistaken in the following way. The word "analytic" is sometimes used by philosophers as a term of logical appraisal, but not always. For instance a philosopher may employ it to show in debate that his opponent has said nothing which need be refuted—"Well, that's analytic", or "But that's a tautology". Again, however, it does not follow that the word "analytic" is always so employed. For instance, philosophers who have argued that mathematical truths are analytic cannot in every case be truly described as appraising or evaluating mathematics. Sometimes they are merely attempting to point out how mathematics differs from (say) the empirical sciences. A fruitful method of doing so is to *classify* the truths of mathematics as analytic, as contrasted with the truths of science, which are synthetic. On the basis of this classification, philosophers may well go on to appraise the theorems of mathematics (*e.g.* they are "not informative", "trivial", "not significant", etc.) but it is clear that such evaluations rest upon prior classifications.

In his paper, Mr. Hamlyn suggests a further thesis which, when interpreted differently from his suggested interpretation, is indeed true. The thesis might be stated as follows:

4. In ordinary language, there is no class of sentences which can be described as "analytic".

The thesis is true if it is interpreted to mean that any sentence, regardless of its grammatical form, may be used to make a significant assertion—*e.g.* "business is business" is not always to be interpreted as a substitution instance of the logical formula "x is x". Or put otherwise, there are no sentences which are always employed only to utter trivialities. But the thesis is false if it is interpreted to mean that what have traditionally been called analytic statements have no use in ordinary language. As I have tried to show by reference to *Hamlet*, the remark "There's ne'er a villain dwelling in all Denmark, But he's an arrant knave", is meant literally; it conveys no information—but all the same, it has a use.

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DEFEASIBILITY

MR. BALMUTH (MIND, January 1956) discussing my note on Value-judgements (MIND, October 1953) expresses the astonishing view that in denying a statement, we may depart from the rules of linguistic usage which govern its assertion. This is what he means by a statement being 'defeasible', and he takes me to task for applying this notion of negation to some statements only, while "uncritically" withholding it from others. But I have been more consistent than that. I have not applied it at all. In no case whatever do I wish to say that what I am denying in denying 'P' may not be 'P'.

However, Mr. Balmuth did fasten on the central point of my argument—a problem of (to use a helpful term recently revived [1]) 'antiphasis', *i.e.* the relation between two statements, one of which is the negation of the other. The point was that an antiphasis of value-judgements does not exhibit the same relation between them as does an antiphasis of judgements of descriptive analysis. A judgement of descriptive analysis cannot be validly denied except in the absence of some condition of a valid assertion of it. A value-judgement, on the other hand, can be validly denied without denying any of those conditions the presence of which is entailed by its valid assertion: it can be defeated by asserting the presence of some additional condition. Although such a defeating condition may not be foreseen (*i.e.* although its absence may not be imposed) by the 'ordinary rules' which govern the assertion, they are yet *the same ordinary rules* which sanction the denial.

This seems to be contrary to Mr. Balmuth's theory of meaning, and of ordinary rules. For him, revision of a judgement on the ground of "a new and unanticipated condition" can apparently mean only one of two things: either the rules of usage are themselves switched in passing from assertion to denial—which is absurd; or they are excessively vague—which is not true. The rules for the use of value-words ('right', 'wrong', 'kind', rude', 'homely', etc.) are not necessarily vaguer than, for instance, Mr. Urmson's rules for grading apples, cabbages, or horses. They are of a different type. The suggestion of a special vagueness is an interpretative illusion. It is due to the attempt, and the failure, to define the meaning of a 'value-sentence' (asserted or denied) in terms of a set of 'characteristic' particulars (present or absent). As we find it impossible to spot such particulars, we are told—a not unfamiliar dodge!—that they are there all the same, only vaguely. But the ordinary rules for the use of value-words are not spotting-rules—a fact which Mr. Balmuth would hardly have missed, had he examined concrete examples, instead of fixing upon the unenlightening "ordinary use of P".

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[1] G. E. M. Anscombe, 'Aristotle and the Sea Battle', MIND (January, 1956), p. 1.

A NOTE ON THE "EMPTY UNIVERSE"

We usually think of a formula of the lower functional calculus as a theorem if and only if (A) it is valid (*i.e.* comes out true for all possible interpretations) in all *non-empty* domains. In effect those who wish to challenge such a notion of validity by contending that such formula do not hold in *all* domains (and hence are not "really" analytic, or necessary, or logical truths) propose to modify this criterion by substituting 'all domains' for 'all non-empty domains'. This modification reflects their suggestion of the "possibility" of an empty universe. In this note I wish to suggest that the phrase "empty universe" is philosophically both empty and insignificant. For, in order to avoid certain logical difficulties, restrictions must be invoked that change the function of the quantifiers in the empty domain. Thus quantification in the empty domain is considerably unlike quantification as ordinarily conceived. The difference, I suggest, is sufficient to warrant our not accepting the proposed modification.

As Quine has recently reminded us,[1] a formula is usually said to be valid in the empty domain only if (B) upon replacement of all its universal and existential quantifications by 'T' and 'F' respectively it becomes a sentential tautology. A criterion like (B), however, raises some problems. Consider the lower functional calculus with the admission of open formulae as theorems. We could then, for the empty domain, construct the following sequence:

(1) $(x) \sim f(x) \supset \sim f(x)$
(2) $\sim\sim f(x) \supset \sim (x) \sim f(x)$
(3) $f(x) \supset (\exists x) f(x)$
(4) $(x) f(x) \supset f(x)$
(5) $(x) f(x) \supset (\exists x) f(x)$
(6) $(x) f(x)$
(7) $(\exists x) f(x)$

It shows that (B) [step (6)] leads to the conclusion that '$(\exists x) f(x)$' is valid in the empty domain. To avoid the contradiction this entails, as long as we acknowledge open formulae as theorems, we must abandon, for the empty domain either (*a*) sentential logic [steps (2), (5) and (7)]; or, (*b*) the validity of '$p \equiv \sim\sim p$' or of '$\sim (x) \sim f(x) \equiv (\exists x) f(x)$' [step (3)]; or, (*c*) the validity of '$(x) f(x) \supset f(x)$' [steps (1) and (4)]; or, (*d*) the idea that the standard inferences corresponding to (*a*), (*b*) and (*c*) lead from valid formulae to valid formulae. None of these alternatives seems acceptable.

The difficulty can be overcome, following Quine, by (α) restricting theorems to closed formulae and (β) modifying $\ulcorner (\gamma)\phi \supset \phi \urcorner$ by re-

[1] W. V. Quine, "Quantification and the Empty Domain", *Journal of Symbolic Logic*, xix (1954), 177-179.

quiring that γ occur free in ϕ. Note that steps (1) and (4) of the above sequence do not violate (β), which is primarily intended to obviate sequences like

(8) $(y)(\exists x)f(x) \supset (\exists x)f(x)$
(9) $(y)(\exists x)f(x)$
(10) $(\exists x)f(x)$

This sequence illustrates how one could go from '$(x)F$' to 'F' where 'F' is any false statement; but it also involves the denial of the validity of formulae like $\ulcorner(\gamma)\phi \equiv \phi\urcorner$ where γ does not occur free in ϕ. In other words *vacuous quantification is not vacuous in the empty universe.* This is an obvious consequence of the fact that any statement can be made into a true one by prefixing any universal quantifier to it—according to (B). In other words, the quantifiers themselves can be turned into singular sentential truth functions (corresponding to a tautological and a contradictory one) for the empty universe :

p	(x)p	$(\exists x)$p
T	T	F
F	T	F

This " use " of quantification together with the kind of restrictions, and their results, needed to prevent logical catastrophy " in " the empty universe thus point up the vacuity of (*a*) the notion of an empty universe, (*b*) the question of the interpretation of formulae in the empty domain, and, in particular, (*c*) the criterion stated in (B).

We may then note three things :

(1) If I am right then the restriction to non-empty domains in classical validity theory is less arbitrary than it might otherwise seem. Thus the potential philosophical significance of this theory is enhanced.

(2) To reject the notion of an empty universe as rather " empty " is not the same thing as to reject the notion of the null class (in a non-empty universe). The legitimacy as well as the significance of the latter is a matter of record. This fact may even be the reason why some philosophical logicians attribute an exaggerated significance to the notions of an empty " universe " and its " logic ".

(3) One may wonder about the " decision " that renders propositions like (P_1) '(x) Red (x)' and (P_2) '$(x) \sim$ Red (x)' true in the empty universe. That is, we may question whether the notion of " everything " has any significance in such contexts. One may even wonder if such propositions are adopted as true on the basis of an implicit analogy to a different situation. Consider the two propositions (P_3) '(x) (Mermaid $(x) \supset$ Red (x))' and (P_4) '(x) (Mermaid $(x) \supset \sim$ Red (x))'. Since there are no mermaids, we hold both to be true, due to the nature of the conditional sign.

It might not be too far fetched to suggest that (P_1) and (P_2) are thought of as true, in the empty universe, by assimilating them to something like (P_3) and (P_4). Some, for example, may unconsciously think of (P_1) as something like 'for anything if it existed then it would be red'. Thus, since there is nothing in the empty universe the proposition is true. One thus would be turning (P_1) into a conditional proposition and arriving at (B) via the "paradox" of material implication. If this suggestion is plausible we may consider attempts to construct a more generalized criterion of validity by modifying (A) in terms of (B) in an even more suspicious light.

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FORMS AND KNOWLEDGE IN PLATO'S THEAETETUS: A REPLY TO MR. BLUCK

In the course of a note in MIND, Vol. lxv, No. 260 (October 1956), Mr. R. S. Bluck says that Plato held that ἐπιστήμη was a 'direct acquaintance' of simple Forms. In the *Theaetetus*, as elsewhere, he *seems* to imply, Plato was concerned with something different from what we ordinarily call knowledge. It was this knowledge by direct acquaintance.

That this cannot be true of the *Theaetetus* (whatever be the case as regards earlier dialogues) can be seen by putting together Socrates' refutation of the theory put forward in his 'dream', with the passage (207a ff.) which discusses whether a λόγος can be represented as an enumeration of the parts of a thing. It is to be noted that there is also a discussion of the enumeration of the parts of a thing in the first passage (203a ff.). The difference between the two passages is that the earlier one uses words like γνῶσις and its derivatives, making no reference to επίστήμη, whilst in the later passage the reverse is true. This systematic use of different terminology suggests that Plato had made a distinction between two senses of 'knowledge'.

If Bluck were right, one would expect Socrates to conclude his refutation of the dream theory by saying that elements are more ἐπίστητος than complexes. But he does not; he says that they are more γνωστὸς. Plato wishes to say that there is no reason for not accepting the view that there are simples of which we have γνῶσις (a direct acquaintance). But the point of the later passage, given what has been already said, is that ἐπιστήμη does not consist of true belief with the possibility of enumerating the parts of a thing *at all* (whether we have γνῶσις of the parts or not). The application of this to Forms is clear. If Forms are simples, and if despite the dream theory we can have γνῶσις of them, we do not have ἐπιστήμη merely by truly believing something about some object and by being able to list the Forms in which that particular object participates. The implication of the later passage with its discussion about the learning of syllables is that we do not have ἐπιστήμη about a thing unless we also know the principles in virtue of which syllables (*i.e.* combinations of simples) are formed. (*Cf.* Wittgenstein, *Tractatus Logico-Philosophicus*, 2.031 and 2.14.) Plato attempts to shed some light on this in the *Sophist*.

There is nothing in all this that suggests that ἐπιστήμη is knowledge by direct acquaintance.

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FREGE'S "THE THOUGHT"

I SHOULD like to correct an inaccuracy in the prefatory note to Mr. and Mrs. Quinton's welcome translation of Frege's *Der Gedanke* (MIND, July 1956). They state that the essay was one of two (the other being *Die Verneinung*) published in the *Beiträge zur Deutschen Idealismus*. In fact, there was a third essay in the series, *Gedankengefüge*, published in the same periodical in 1923. These three essays represent the opening chapters of Frege's last attempt to write a book covering the whole field of philosophy of logic. There is a fourth chapter, which survives in typescript form in the late Professor Scholz's collection of Frege's unpublished writings at Münster, Westphalia. Another contemporary manuscript shows that Frege had not at that time abandoned the Sinn-Bedeutung distinction, as one might suppose from the lack of mention of that distinction in the three published chapters. Frege made six attempts during his life to write such a comprehensive book on his views on philosophy of logic, the first in 1880; he never completed any of them.

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VIII.—CRITICAL NOTICES

Remarks on the Foundations of Mathematics. By L. WITTGENSTEIN. Edited by G. H. VON WRIGHT, R. RHEES and G. E. M. ANSCOMBE. Translated by G. E. M. ANSCOMBE. (Basil Blackwell, Oxford.) Pp. 400. 37s. 6d. 1956.

THIS volume contains selections from manuscripts written between 1937 and 1944. As in Wittgenstein's first book, and in the posthumously published *Philosophical Investigations*, the original German and the English translation are printed side by side, the original text occupying nearly 200 pages. The volume is divided into five parts.

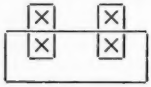
The first part, written during the years 1937 and 1938 is a fine example of Wittgenstein's method and contains many remarkable observations. One of the principal problems discussed is the use of a sign as the mark of a concept. The reduction of the problem of the meaning of words to the study of the usage of signs leads very readily to a confusion of the various ways in which we may look at the use of a sign. Wittgenstein illustrates this in terms of geometrical figures. A figure may be intended to represent the parts of a machine, and be prepared by a draughtsman for use by a mechanic who is making the machine; the mechanic will make use of certain *physical* properties of the drawing which he must measure and copy in metal. But a figure may also serve to draw attention, not to itself but to some general feature of a situation. Thus, for instance, although measurement of an accurate drawing plays no part in a proof in geometry, nevertheless a figure may constitute (part of) a proof when it serves as a notation in which the proof is expressed. Wittgenstein gives the example of a drawing of a pentagon with its vertices linked to the five fingers of a drawing of a hand. This drawing *may* be regarded as the result of an experiment—the experiment of correlating *this* pentagon (physical object) with *this* hand. And if it is so regarded it has nothing to teach us about mathematics. The correlation is in space and time and may cease to hold tomorrow. But the figure might also be used as an *illustration* of the concept of the correlation, or as a *representation* of the correlation of the points of a pentagon (mathematical concept) with a hand (concept). This last is what Wittgenstein calls using a sign as the mark of a concept.

Again and again he seeks to draw the distinction between an experiment and the application of a rule, stressing the fact that a mathematical rule cannot *compel* the result of an experiment. The rule $2 + 2 = 4$, *qua* rule is eternally valid because it is outside time, but in a world in which no objects persisted the rule would lose—not its validity but its use. Thus (37) " Put two apples on a bare table, see that no one comes near them and nothing shakes

the table ; now put another two apples on the table ; now count the apples that are there. You have made an experiment, the result of the counting is probably four. . . . If the result at one time were 5 and at another 7 (say because, *as we should now say*, one sometimes got added, and one sometimes vanished of itself) then the first thing we said would be that apples were no good for teaching sums. But if the same thing happened with sticks, fingers, lines and most other things, that would be the end of all sums. ' But shouldn't we then still have $2 + 2 = 4$ '—This sentence would have become unusable."

The immediately following remark (38), one of the most penetrating in the book brings out that aspect of the use of a figure. " You only need to look at the figure

to see that $2 + 2$ are 4 — Then I only need to look at the figure



to see that $2 + 2 + 2$ are 4."

Another question to which he returns repeatedly is " In what sense is logical argument a compulsion ". If inference may be analysed in terms of sentence forms, as when we say that q may be inferred from p and $p \rightarrow q$, does this make inference a property of signs ; or is it perhaps a matter of psychology and we infer as we do because our minds are so constituted ? Thus (133) : " The propositions of logic are ' laws of thought ', ' because they bring out the essence of human thinking '—to put it more correctly, because they bring out, or show, the essence, the technique of thinking. They show what thinking is and also show kinds of thinking."

Then later (141) : " What we are supplying are really remarks on the natural history of man, not curiosities however, but rather observations on facts which no one has doubted and which have only gone unremarked because they are always before our eyes." And perhaps most clearly of all (152) : " What does people's agreement about accepting a structure as a proof consist in ? In the fact that they use words as *language* ? As what we call language ? " (161). " If a proof is conceived as an experiment, at any rate the result of the experiment is not what is called the result of the proof. . . ." (163) " Certainly experience tells me how the calculation comes out ; but that is not all there is to my accepting it." (164) " So much is true about saying that mathematics is logic : its movement is within the rules of our language. And this gives it its peculiar solidity, its unassailable position, set

apart." (Mathematics deposited among the standard measures.) (167) "The mathematician is an inventor, not a discoverer."

Part I is followed by two appendices; the first of these discusses an informal version of Gödel's non-demonstrable proposition. Apart from the very important observation (7) "'But may there not be true propositions which are written in this (Russell's) symbolism but are not provable in Russell's system?'—'True propositions', hence propositions which are true in *another* system, *i.e.* can rightly be asserted in another game, . . .; and a proposition which cannot be proved in Russell's system is 'true' or 'false' in a different sense from a proposition of *Principia Mathematica*", this appendix is unimportant and throws no light on Gödel's work. If the date of this appendix is 1938 as the preface tells us then we are faced with the mystery that what Wittgenstein said on the subject in 1935 was far in advance of his standpoint three years later. For Wittgenstein with remarkable insight said in the early thirties that Gödel's results showed that the notion of a finite cardinal could not be expressed in an axiomatic system and that formal number variables must necessarily take values other than natural numbers; a view which, following Skolem's 1934 publication, of which Wittgenstein was unaware, is now generally accepted.

The second appendix discusses the concepts of set theory. It is argued that Cantor's famous proof of the non-denumerability of the real numbers shows that every sequence of real numbers determines a new number, not in the sequence, and does not establish the existence of a non-denumerable class. Thus (7): "It might be said: Besides the rational points there are *diverse systems* of irrational points to be found in the number line. There is no system of irrational numbers—but also no super-system, no 'set of irrational numbers' of higher order infinity."

On the infinity of a sequence we find the illuminating remarks (12) "To say that a technique is unlimited does *not* mean that it goes on without ever stopping . . .; but that it lacks the institution of the end, that it is not finished off." (17) "'Ought the word "infinite" to be avoided in mathematics?' Yes; where it appears to confer a meaning upon the calculus; instead of getting one from it."

Part II, which is dated 1939-40, is fragmentary and much of it is in an unfinished state. A question, or a thought, appears again and again in diverse forms, as Wittgenstein seeks to clarify his ideas and achieve a significant phrase. This makes many passages hard to read, but there are many striking clues to be found, enough to make the search worth while. The fundamental problem again is the antithesis of experiment and calculation. This is a subject which had engaged his attention for many years. In a lecture in 1931 he touched on the same theme when he said that one may devise an invention which won't work but one cannot make a game which won't work. Reminded of this remark several years later

he at once retorted that of course one can make a game that won't work, for instance chess played with pieces weighing over a ton each. These two ways of using the notion of the operation of a game, the physical operation and that which belongs to logic are precisely what he is trying to bring out in this part of the work. Another theme in this part, not always clearly brought out, is the problem of the relations of notions to notations. Thus he says (4) "The *application* of the calculation must take care of itself, and that is what is correct about formalism." And in (29) he asks "Could we say: mathematics creates new expressions, not new propositions", and again in (47), "'It is only a different notation'. Where does it stop being—just another notation'. And he answers 'Isn't it where only the one notation and not the other can be used in such and such a way'." And perhaps one might add, when the new notation sums up the problem in a nutshell. This of course is related to the view Wittgenstein has often expressed that if you wish to know what a proof proves, look at the proof. Thus we find (1) "a mathematical proof must be perspicuous", and (26) "Nothing is more likely than that the verbal expression of the result of a mathematical proof is calculated to delude us with a myth."

Towards the end of this part he takes up again the problem of contradiction. A contradiction may arise from a new way of looking at a familiar situation, or from a new way of applying a rule. In straight streets an arrow pointing to the left leads to a different goal from an arrow pointing to the right, but if the street is circular . . . . "I have invented a game", he says in (77), "—realize that whoever begins must always win: so it isn't a game. I alter it; now it is alright." Only in the theory of sets we are never certain that it is alright. In the midst of these considerations we meet this important general observation on philosophy: "It is often useful, in order to help clarify a philosophical problem, to imagine the historical development, *e.g.* in mathematics, as quite different from what it actually was. If it had been different no one would have had the idea of saying what is actually said" (80). The second part closes with the tantalising observation "I have not yet made the role of miscalculating clear. The role of the proposition: 'I must have miscalculated'. It is really the key to an understanding of the 'foundations' of mathematics."

Part III, dated 1942, returns again to the problem of formulating the distinction between a mathematical and an experiential proposition, and the nature of following a rule. This part is just a fragment and does not appear to break any new ground. One interesting observation is that if calculating looks like the action of a machine, it is *the human being* doing the calculation that is the machine. At the end there are some remarks on contradiction, suggesting possible *uses* for a contradiction.

Part IV is chiefly concerned with the problem of finitism in mathematics. The position is still that which Wittgenstein held

ten years earlier. The application of the *tertium-non-datur* in such contexts as "a recurrence of 3 sevens either does or does not occur in the decimal expansion of π" is rejected on the grounds that the 'decimal expansion of π' is not something in being, but a rule of procedure; π is likened to a character in a novel that has not been completed. Thus (20): "Only within a mathematical structure which has yet to be erected does the question allow of a *mathematical* decision."

The most important question raised in this part is that of the *sense* of a mathematical proposition, and what is meant by understanding a mathematical proposition. The view that understanding is not involved because mathematical propositions are only positions in a game, is sharply rejected as nonsense. "It is essential to mathematics that its signs are also employed in *mufti*. It is the use outside mathematics, and so the *meaning* of the signs that makes the sign game into mathematics."

The last part, dated 1941 and 1944 returns to the problem of differentiating between calculation and experiment, which clearly dominated Wittgenstein's thinking, and has some further remarks on the *sense* of a mathematical proposition and on contradiction; the sense of a mathematical proposition is to be found partly in the proof of the proposition, and partly in its application, but we are not told what the *application* consist in. "Should we say", he asks, "that people possessed a mathematics if they used calculating, operating with signs, *merely* for occult purposes." On contradiction he says, "if a contradiction were now actually found in arithmetic—that would only prove that an arithmetic with *such* a contradiction in it could render very good service; and it will be better for us to modify our concept of the certainty required, than to say it would really not yet have been a proper arithmetic."

One of the questions which will be asked about this book is what contribution does it make to the problems with which workers in the foundations of mathematics are today concerned.

The answer is certainly that it makes no contribution whatever. And this is because, as Wittgenstein himself says, the problems with which they are concerned are mathematical, not philosophical problems. The questions which Wittgenstein answers are those which he himself raises so skilfully, and which to the logician and mathematician are perhaps not questions at all. Another question which will be asked is what this book will do for Wittgenstein's standing in the world. That it contains gems of insight and wisdom is indisputable; that it lacks the polish and finish that he himself so much sought and admired is equally certain. On balance we must be grateful to the Editors for the wealth of material they have brought together. A special word of thanks is due to Miss Anscombe for a translation which is so apt that it reads like Wittgenstein's own superb spoken English.

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The Language of Modern Physics. An Introduction to the Philosophy of Science. By ERNEST H. HUTTEN. London : George Allen and Unwin Ltd. New York : The Macmillan Company, 1956. ' Pp. 278. 21s.

PHILOSOPHERS of science are divided into two broad schools. On the one hand, there are the historicists, who appear to think that the subject is concerned with little more than the historical exploration of the ways in which particular scientific discoveries came to be made, and, on the other, there are the methodologists, who see its task as that of laying bare the logical principles involved in these discoveries and the logical structures of the resulting theories. The historicists tend to accuse the methodologists of producing accounts of scientific methods which look very unlike the actual procedures of the scientist : the methodologists tend to reply that the historicists confuse psychology with logic.

There is some justice in both criticisms and something to be said for both approaches, but the two are not irreconcilable. A study of the history of science can undoubtedly assist an accurate logical analysis, if only by keeping before us science as it is rather than science as we imagine it to be ; our understanding of science as it is can be greatly increased by logical analysis of its concepts and theories. If we are interested in understanding scientific theories rather than scientists it is difficult to reject the approach of the methodologist and the conclusion that logical analysis is the end to which philosophers should regard the history of science as a means. However interesting the psychology of discovery may be, it is not the philosophy of science. Moreover, it is no criticism of a logical analysis that it is not a description of what is analysed.

Dr. Hutten is a methodologist who would, I think, agree with this and whose approach I therefore find sympathetic. He is also a working physicist and so is both anxious and likely to give an interpretation of science which will do justice to its real nature. He, says " the discovery of scientific theories, interesting as it may be, must be distinguished from their logical justification ", and it is with their logical justification that his book is concerned. It is intended as an introduction to the philosophy of science and aims to give a " logical reconstruction " of scientific method as we know it today. He casts it in the form of " a semantic analysis of the main concepts of physics and of scientific method ". Because physics is the most advanced of the natural sciences, and its results the most strongly confirmed, he takes physics as " best fitted to serve as an example of the ideas of modern science".

The book contains, after a short introduction, a chapter on some of the concepts of logic, with special reference to the distinction between syntactic and semantic rules, and chapters in which these concepts are used in discussions of the concepts of classical physics, thermodynamics and quantum physics. It concludes with a long

chapter on "Scientific Method and Semantics", in the course of which Hutten draws the threads together into a semantic-syntactic interpretation of the status of theories, laws and hypotheses and says some penetrating things about induction and probability. The book would have been improved by the inclusion of references and a bibliography, which it entirely lacks, and a fuller index.

A general criticism of the book is that Hutten seems to be uncertain to whom he is introducing the subject. A work with such a sub-title might, after all, attract philosophers who know little science, scientists who have read little philosophy and laymen who know little about either. It is a formidable task to write for all these. Readers from each group will find much to interest them, but all of them may find it unsatisfactory at some point. Some of the logical and scientific terms may not be clear to the reader coming to them for the first time. The scientist is likely to find the discussions of some scientific concepts, such as "force" and "energy", too elementary and the non-scientist to find the discussion of quantum physics too difficult. This is a problem perennially facing the writer on "borderline" subjects; he must perhaps be content to address a more restricted audience.

Apart from this, the chapters entitled "Quantum Physics" and "Scientific Method and Semantics" are stimulating and valuable, especially as there is hardly any work published in England which deals with the subject from Hutten's point of view. I am inclined to think that the most valuable feature of the book is his account of the use of models in physics. A model, he says, is part of the interpretation of the theory. As such, he argues, against the extreme formalists, models are logically, and not merely psychologically, necessary to give content to the abstract expressions of the theory, to give them an application to the phenomena. Attempts to regard modern physical theory as purely mathematical are misguided, since such a theory could not possibly describe our experiments. The mathematical formalism must be given an interpretation; the model does this in terms either of an earlier theory or of familiar everyday concepts. The most abstract of modern theories can be seen to need at least such terms as "wave-function", but to apply this to an expression is to remove it from the field of pure mathematics by the use of a model, attenuated though it is. The model is a link between theory and experiment without which we should not know how to experiment, or, at least, how to make our experiments bear on the theory being considered.

However, having praised Hutten's conception of the philosophy of science and admitted the merits of important sections of the book, I am bound to say that I find some of his conclusions, and the arguments for them, unsatisfactory, perhaps because limitations on space have prevented his discussing them more fully. I must restrict myself to some comments upon his central conclusions about the relations between theories, laws and hypotheses.

His view, in the tradition of Ramsey, Carnap and Reichenbach, may perhaps be summarized in this way. A theory is a description of experiments and consists of at least two parts, a formal calculus and a system of semantic rules which provide its interpretation in terms of experience (p. 36). It is a " language system ". A law is not simply a general statement which can be said to be true or false independently of a theory ; it only makes sense when it is understood as belonging to a given theory, within which it is analytic. It is " part of . . . the semantic skeleton of a theory ". It is " a factually empty statement within the theory " or " a statement belonging not to the theory but to the corresponding metatheory " (p. 235). We cannot say that theories and laws are probable or that we confirm them. We can say these things only about hypotheses, which are derived from laws, presumably, although Hutten does not dwell on this, by deduction. Putting it in another way, we fill in the empty schema, which is the law, with initial and boundary conditions and so obtain a hypothesis which is tested by experiment. Laws are universal in scope but hypotheses are specific existential statements. A law, indeed, is *testable* by instance confirmation but what is *confirmed* is the hypothesis derived from it. The theory is adequate if the hypotheses derived from it describe and predict the phenomena. There are two points here which need clarification.

First, a theory is said to be both a language system and a description of experiments. But can it be both? A language system, a collection of syntactic and semantic rules, surely cannot *describe* anything. A collection of rules describes nothing ; it announces our intention to talk in a certain way. It is only sentences formulated in accordance with these rules which can describe. The set of rules implicitly contains all the things we may say, but just because it contains them *all* it cannot say any one thing rather than another, which a description must do. Yet surely a scientific theory is in some sense specific, in some sense says certain things rather than others it could say and does not. At least there are sentences which form part of it which say certain things and do not say certain other things about, for example, the ways in which bodies move. It determines not only the way we talk but that we shall say that certain quite specific statements are true, others not. This must be so if we are ever able to reject a theory in the light of evidence, as Hutten admits that we do. I shall return to this.

Second, Hutten says that a law is a sentence *within* a theory and is *part of* a theory (p. 248), and he speaks of a hypothesis as " derived from a law " (p. 237), " derived from a theory " (p. 236) and a " sentence within a theory " (p. 235). Now, a hypothesis is specific, confirmable, directly about phenomena and so not just a rule about language but a statement about experiments. Yet if it is within a theory, part of a theory, one is tempted to say that the theory which contains it cannot be just a language system but makes statements in the language.

But here there is perhaps some danger of confusing what might be called "strong" and "weak" senses of the words "within", "part of" and "contains", which have a serious ambiguity in this context. Any English sentence is, in the weak sense, within, and part of, the English language, *i.e.* formed according to its rules. But if it was raining at noon yesterday my statement "It was raining at noon" will be within, and part of, my description of yesterday's weather, in the strong and quite different sense. There is no specific thing which you must say to be speaking the English language correctly, but there are certain things you must say to give a correct description of yesterday's weather. The things you do not say in giving the correct weather report are not part of the weather report (strong sense); many of the things you do not say while speaking English are still parts of the English language (weak sense).

Now if a hypothesis is within a theory in the strong sense the theory cannot be said to be simply a collection of rules or a language system, but it can be said to be descriptive. On the other hand, if every hypothesis is within a theory merely in the weak sense I do not see how the theory can be said to be descriptive. Hutten seems usually to be using "within" in the weak sense, as indeed he ought if he wants to say that a theory is a language system and that hypotheses are *derivable from* the theory, but I cannot see how he can avoid the strong sense if he wants to say that theories are descriptions of experiments.

I should have thought that it was normal to regard scientific theories as containing hypotheses in the strong sense and therefore correct to regard them as descriptive. This would make it incorrect to say that a hypothesis was derived from a theory but correct to say that it was derived from some of the higher-level statements of the theory.

Here we are being forced towards recognition of another difficulty in Hutten's account. If a theory is a language system can hypotheses be said to be *derived from* it? It is surely not correct to speak of deriving statements *from* a system of rules but only *in accordance with* such a system. Of course, there are different methods of derivation, but Hutten seems to have deduction in mind as at least one of the methods of derivation, since he says that "physics pursues the hypothetical-deductive method" (p. 226). I do not want to deny that theoretical statements sometimes function as rules but neither can it be denied that they sometimes function as premisses. His awareness of this fact is perhaps what leads Hutten into saying that hypotheses are derived from theories against what seems to be the drift of his account.

In supporting his view, he leans heavily upon an argument which purports to show that we cannot speak of a law being probable or confirmable. Laws, he says, "being universal in scope, cannot be said to be probable since an infinite number of instances alone would suffice to confirm them; and any finite number of instances can

only result in zero probability (or degree of confirmation): that is, the concept of probability does not apply to laws" (p. 234). Because hypotheses are "very specific sentences" evidence can render them probable.

The assumption that to say that a scientific law is universal in scope means that it refers to an infinite number of instances is one which has frequently bedevilled discussions of this sort. As Hutten says in a later section, "it is not a factual question to ask for an infinite number of confirming instances: infinity is a concept that has logical, but not physical, meaning". Quite so, but need we conclude, as Hutten does, that laws, being universal, cannot be rendered probable or confirmed by evidence and can only be analytic? Might we not conclude that scientific laws are not universal in this sense but only in the narrower sense that, if they are put in the form "All S . . .", they refer only to all S's which have existed, do exist and could exist? It seems likely that this is what we do mean, considering the context in which scientific laws are stated. And does it make sense to say that an infinite number of S's could exist? Any number of S's which existed would be in principle countable and so finite. The universality of laws does not necessarily show that they are analytic. If they are universal only in this narrower sense they may perfectly well be said to be confirmable.

In support of his view that laws are analytic Hutten puts forward another argument which might better be taken to support the opposite view. He points out that when a law has been used successfully and then is found wanting we tend to keep the law, because it is useful, but we restrict its range by embodying it in a new theory of modified range. He instances the modification of Newton's Second Law of Motion in view of the development of Relativity Theory. He says "Though, strictly speaking, the meaning of the sentence (*i.e. the law*) is changed, this is shown only by placing it within a different context, or a more restricted theory, and not by changing the sentence" (p. 234). This, he says, "suggests that a law is presented as an analytic sentence within a theory". But is there really any relevant difference between "changing the meaning of a sentence" and "changing the sentence"; between modifying or rejecting a law in the face of evidence and restricting its scope by restricting the range of the theory?

Hutten admits that, since a universal sentence is equivalent to the negation of an existential sentence, a law may be rejected in the face of a single negative instance. This suggests, although I do not know whether he would like to say this, that a law may be disconfirmed but not confirmed. This seems odd. His discussion of Newton's Second Law brings out the fact that it had to be restricted in scope because it was found not to apply to fast-moving bodies. But if it fails to describe only certain motions it must succeed in describing others, and it seems odd to deny that these others confirm

it. If what I have said about the universality of laws is satisfactory there is no need to deny it.

If we keep the law and restrict the range of its theory we can, perhaps, say that the law is analytic within the theory. We are used to thinking that if a sentence is analytic it does not make sense to talk of confirming or disconfirming it, or even rejecting it in the face of negative instances. But Hutten's analytic sentences behave differently. If we say that scientific laws are analytic within a theory and yet may be restricted in the face of evidence by a restriction on the theory this looks very like saying *either* that a sentence can be analytic and yet confirmable, or at least disconfirmable, *or* that a theory is disconfirmable. If we also say that theories are sets of rules we find ourselves in an odd situation. " All bald men are bald " is analytic as long as we accept some rule to the effect that a word is to have the same meaning on each appearance in a given context. It is neither confirmable nor disconfirmable because no possible experience, so far as I can see, could lead us to reject this rule. It is true that we might reject it if we found that it led us into contradictions or that we failed to understand one another, but we could not be forced to reject it by an examination of bald and hirsute men. But evidence may lead us, as Hutten admits, to modify a scientific theory, as the Newtonian Theory was modified in view of a consideration of fast-moving bodies. This, on his account, is to reject or change a rule. It looks as if some rules fit our experiences while others do not, and we are concerned to find those that do. This, to say the least, is an unusual conception of a rule. If a law can fit or fail to fit certain phenomena and if a theory may be restricted in view of the evidence this surely suggests that a law cannot be a " factually empty statement " and that a theory cannot be just a set of rules.

In more than one section of the book, Hutten appears to be torn between a sound understanding of the structure and use of scientific theories and the requirements of a semantic account of them. This appears to lead him into inconsistencies which suggest that there is something fundamentally wrong with this kind of account. I say " appears " because it may be that my objections have no substance and could be removed without difficulty. If this is so, then Hutten has not made clear his conception of a language system, which is a pity since so much depends upon it.

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IX.—NEW BOOKS

Conceptual Thinking : A Logical Inquiry. By STEPHAN KÖRNER. Published for the University of Bristol at the University Press, Cambridge, 1955. 30s. net.

THIS is an extremely puzzling book. It is certainly as difficult to read as any philosophical work I can readily recall ; and I mean " difficult to read ", not merely " difficult to understand ", though doubtless the one implies the other. This is partly because the rate at which large questions are disposed of is bewilderingly, and in the end exhaustingly, rapid ; partly because one finds in almost every sentence familiar words used in unfamiliar ways, so that incessant effort is demanded not to take what one reads in the ordinary sense, but in the special sense assigned at some earlier point. This operation, constantly performed, becomes very tiring ; and the text, from one end to the other, is wholly relentless.

But the book is also puzzling. In it ordinary concepts are neither employed nor much discussed ; yet it is not equipped with the full formal panoply of a constructed language, and is evidently intended to bear on many problems considered by philosophers who have made no use of Professor Körner's vocabulary. Now it may be that these problems can be stated in this vocabulary ; but if the effect of this re-statement is, as it very often appears to be, that the problems are made to vanish very quickly, gratification at this result must inevitably be tempered by the suspicion that something of interest has been lost. More generally, the method applied to problems in this book could be characterized as " nearly-formal " ; and there is the suspicion that those who employ this method are leaving one stool without quite reaching another.

The announced subject of the book is this (p. 4) : " We are to consider the rules of conceptual thinking ; relations between these ; and some contexts in which they can be accepted or satisfied." The terminology to be employed is that of " concepts and propositions ". And at once we run, I believe, into serious difficulties. It appears that in conceptually thinking we use " signs ". If (p. 12) in using any sign we satisfy " certain rules ", we are said to be using the sign " as a statement ". If there is another sign, the rules governing the use of which differ only in that they govern the use of this different sign, then this and the former sign are " synonymous ". Now if we so use either sign as that " arbitrary replacement of it by any of its synonyms is allowed ", we are said to use it " as a proposition ". Predicates and concepts are said also to be signs, their use governed by " certain rules " ; and rules in turn (p. 18) are said to be propositions. Now if all these things—statements, concepts, predicates, rules, etc.—are signs, how do we distinguish one from another, or any from signs which are none of these things ? Here, it seems to me, things begin to fall apart in a most curious way. We are told, for instance, by way of distinction, that some concepts " apply " to what are called " bases ", whereas no propositions do. But we are not told what " applying " a concept (*i.e.* a sign ?) is. Are we expected to know this ? Further, it being thus unclear what a proposition is not, we read (p. 17) that " only

a proposition . . . entails or is entailed ". But is it not extraordinary that, the notions of " proposition ", " statement ", etc., being judged to require elaborate explanation, we should be assumed to understand quite well the notion of entailment ? And do we in fact understand the notion of entailment, if those things between which entailments are to hold are signs governed by " certain rules ", and we are told by implication that statements do not entail nor are entailed ? What we commonly mean by " entailment " seems doubtfully relevant to the explanation of a special and peculiar use of the words in question ; but what Professor Körner means by " entailment " is not made clear. Hence we are involved at once, in my submission, in the characteristic predicament of this book ; the use of the key words is both odd and incompletely explained, so that one is forced frequently to refer to one's pre-existing understanding of them—with, however, the uneasy conviction that this reference is officially disallowed. Certainly my impression is that, in the early pages, it would be impossible to discern what was being talked about, unless the vague references to " signs " and " certain rules " were filled out by an ordinary understanding of what it is to use words in making statements, applying concepts, and so on ; yet the definitions which one is obliged to fill out in this way are intended to be of special uses of these words ; so that there seems to be an uncomfortable conflict between what one is actually told, and what one has to bear in mind if what one is told is to be comprehensible. This seems to me to be the consequence of abandoning one foothold—the ordinary meaning of words, without quite reaching another —full, formal definition of technical terms.

The adopted terminology having been thus explained, or nearly-explained, in the opening pages, there follow short discussions—about ten pages each—of a large number of problems in logic, epistemology, ethics, and metaphysics. Many interesting and often ingenious points are made, but the general effect continues to be puzzling. I shall pick out one instance, the discussion of truth, the intention of which is to me particularly obscure. Professor Körner begins (p. 198) by suggesting that there is at least on thing common to all non-technical uses of " true " : " to state that a proposition is true is to imply that it is appropriate." And a proposition is appropriate if " whoever were to apply the concept which is applied in stating the proposition, to the basis to which it is therein applied, would not violate the rules governing the concept ". Now if any misapplication of a concept violates " the rules governing " that concept, it would seem to be somewhat unadventurously true that any true proposition is thus " appropriate ". But on page 200 it is further suggested that this notion of appropriateness may be regarded as a definition of truth ; and this surely, as well as being unilluminating, is highly debatable. For if this is correct, then one who says that a proposition is true not only (as was said at first) implies that it is appropriate, but says that it is appropriate ; and this is more than doubtful. It is not, however, further discussed. Instead the correspondence theory is stated in one sentence, defended against two obscure objections, and then dropped ; and there is a longer, ingenious, but inconclusive discussion of some coherence theories. The chapter then ends. Now what is the purpose of this ? The ordinary use of " true " is discussed hardly at all ; the different question, what it is for a proposition to be true, is answered trivially ; traditional theories are glanced at with bewildering perfunctoriness ; but a concern with artificial, formalized definitions is officially disclaimed. Consequently, I do not see what the chapter is for, what

light it throws on anything at all. There are of course many chapters which do not leave this odd impression; but there are many which do.

It will be clear that I have not understood this book, though I can see that it is a scrupulous, clever, and even a subtle piece of work. My difficulty is that I cannot discern its purpose. I cannot make out what problems they are that could properly be dealt with in this way, with such dispatch and contrivance and in such large numbers. But in justice to Professor Körner I should wish to emphasize that I do thus fail to understand him. I am entirely ready to believe that this is largely my own fault, due perhaps to inability to cope with the very unfamiliar; and it may well be that other philosophers will learn a great deal from what is, to me, so thoroughly baffling.

G. J. WARNOCK

Hopi Ethics: A Theoretical Analysis. By R. B. BRANDT. Chicago University Press and Cambridge University Press. Pp. x + 398. £2 16s. 6d.

THIS book marks out a novel approach. The author is a moral philosopher with certain views on the phenomenology of ethics which he wants to test, and he tries to do this by considering them in relation to the ethics of a primitive society, the Hopi, a Pueblo Indian people of northern Arizona. Professor Brandt did his own field work; it differs from field studies as carried out by most British anthropologists in being mainly based on questions to informants, to elicit the sorts of things they thought right and wrong and what qualities they approved and disapproved and why; it is not based on descriptions of behaviour in certain typical situations, nor on detailed description of the structure of Hopi society, for instance of their system of familial relations. Professor Brandt refers in his bibliography to studies of the Hopi by professional American anthropologists; but he cites these mainly as giving evidence for psychological traits. He does not, as I think most British anthropologists would, try to see how these traits might be connected with the structure of Hopi society. This seems to me to affect his analysis in certain respects, which I shall try to indicate.

Brandt wants to see whether the moral outlook of the Hopi supports certain views of the relation between ethical attitudes and beliefs. An attitude is defined in dispositional terms: to say X has an attitude to some state of affairs S is to say that he would experience a certain typical affective reaction if S were to occur. An attitude is called ethical if it is a feeling of obligation or a judgment about a voluntary act, motive or trait of character supposed to be at least partly under the control of the person judged; and also if it is *disinterested*, by which is meant not only that it must be independent of consequences to oneself as such, or of attachments to particular persons as such, but also impartial towards particular social groups and statuses. A person is said to be impartial only if he has a strong and equal concern about the welfare of *all* persons. Here I wonder whether Brandt could have laid down quite so universalist a condition if he had taken more account of the structural analysis of Hopi society. This would have shown how social relationships obtaining

between different groups and categories of persons would give rise to special degrees of obligation and consideration; is this the same as "prejudices towards social groups and statuses"? Such studies make it abundantly plain that the morality of primitive societies, indeed probably of all our societies, is largely a morality of "My Station and its Duties". There are distinctions between members of "in-groups" compared with "out-groups"; these need not mean that no obligations to strangers are recognized, but that kinds of obligation are closely geared to social relationships. Possibly one sign of ethical advance (if we want to talk about this) may be that these distinctions count for less; but surely they are always likely to count for something. The completely universalist saint would have to cut himself off from family ties. This point is worth mentioning, since depreciation of the "role and status" kind of morality, though elevating an abstract ideal of universal impartiality, may make us unrealistic about the morality of actual societies. Brandt also mentions that his enquiries about Hopi ideals of personality were put in quite general terms; informants were simply asked whether something was a good trait to have or not. Possibly his informants might have been more forthcoming if he had discussed with them what traits were fitting for certain types—the good chief, the good husband, the members of different age groups. Such reading as I have done of anthropological literature suggests that primitives naturally think of morality in these specific terms.

Brandt is interested in the general question of whether attitudes can be said to be causally dependent on beliefs. Tentatively, he thinks attitudes may be explained by the "theorem of utility", defined widely as belief that certain kinds of acts have favourable or unfavourable effects of a kind which fall within the personal or vicarious experience of the agent; and/or by the "theorem of reward", *i.e.* that the holding of certain attitudes is found to be rewarding, *e.g.* in winning social approval. This latter would encourage people to cultivate the attitudes; it would not explain the approval, and presumably the weight of this would have to be carried by the "theorem of utility". The simple emotive view of terms of approval and disapproval—that "This is wrong" is to be analysed simply as "I don't like it"—was received with shocked horror by Brandt's Hopi when it was put to them. The reasons they gave for thinking something was wrong were interestingly heterogeneous, and very much what we in our Western tradition would call "sensible". They are certainly not just based on custom and taboo (as a ritual prohibition for which no reason is given); a point stressed by Professor Macbeath in his *Experiments in Living* and evidenced by Malinowski and others. Malinowski indeed represented the social ethics of primitive peoples as quite calculatingly utilitarian. This is not borne out by some of the reasons given by Hopi. For instance: "How do you come to think adultery is wrong?" "I think adultery is wrong because you see feelings will be hurt when you are found out. After all the hard work the man has done for her." Here it looks as if considerations of sympathy and fairness were ingredients in the attitude. Can these be said to be causally dependent on expectations as indicated by the "theorem of utility"? Sympathy might be got in, as it was by the older utilitarians, as the desire to avoid vicariously unfavourable effects; fairness ("after all the work he did for her") is less easy to explain by reference to expectations of future effects.

Brandt discusses whether anthropological evidence can help decide what he calls "causal relativism" in ethics. This is distinguished from "logical relativism": the question of whether "A is right" and "A

is not right" said by two speakers are contradictories. If "right" is to be analysed in propositional terms, *e.g.* as "will increase the happiness of mankind", then there would be a contradiction; if it is to be analysed as including an attitude, there need be no contradiction. To decide this is said to be a matter of establishing the usage of ethical terms in our Western tradition; indeed the strictly *philosophical* questions of ethics are said to be confined to this, so that anthropological evidence would not have much bearing on them, except in so far as it might show how far the Western tradition is typical. (This is surely rather parochial; even on the assumptions of the school of Usage, why should there not be philosophical questions for Hopi moral philosophers getting to work on Hopi usages?) Brandt thinks, however, that anthropological evidence might be relevant to a decision on *causal* relativism. This, it is said, could be established if two persons or groups, both of whom knew all the relevant facts and were equally "qualified" were found still to differ in ethical attitudes. "Qualified" is taken to mean that a person must be sane, possessed of correct beliefs on the facts in a maximally clear and vivid form, not be ill, not in a condition of physical craving, fatigued, excited, nor depressed, and have a strong and favourable sentiment toward all human beings and no biasing towards social groups or statuses (p. 240). Brandt sees that these qualifications are put in ideal terms, and may be comparable to questions about frictionless planes. Yet, even as an approximation, such a test would surely be extremely difficult to administer. Who is to judge that both parties hold their beliefs in a maximally clear and vivid form; or even that they are correct as to all relevant facts? Perhaps, however, it will serve if we say that they should have the same beliefs about the same number of facts. Even so, how can we be sure that there is not some qualification overlooked, and possessed by one party and not the other; and unless the qualifications are to be held in an absolute degree (surely a hard condition for an experiment on relativism to have to satisfy), how can we be sure that differences in degrees of their possession may not causally affect the attitudes? If the test is to be made not between two people of the same culture, but between two different ethnological groups, then judgments about facts, made in the context of different intellectual and cultural backgrounds, might be very hard to equalize. Brandt tried out a comparison of attitudes between the Hopi and a group of Swarthmore students on a number of questions such as certain kinds of sex relations, the use of alcohol, and aggressive behaviour. He found differences of attitude could on the whole be correlated with differences of factual beliefs, except in the case of cruelty to animals, which Hopi disapprove of much less strongly than white Americans, in spite of the fact that their beliefs about animals assimilate them more closely to human beings. This seems to Brandt possible evidence in favour of causal relativism. It also seems to suggest that not only the entertainment of beliefs, but also the cultivation of sympathy, encouraged by our Western moral and religious tradition, can causally affect attitudes. This brings us back to the question of whether sympathy and fairness, which seem to involve both feeling and cognition, including imagination, are not rather special ingredients in ethical attitudes, and whether they are catered for sufficiently in Brandt's analysis.

These are only a selection of the issues raised in this most interesting and original book.

DOROTHY EMMET

Essays on the Law of Nature. By JOHN LOCKE, edited and translated by W. VON LEYDEN. Oxford: The Clarendon Press, 1954. Pp. xii + 292. 35s.

THIS volume consists of three parts. First, there is a long Introduction by Dr. von Leyden about the Lovelace Collection of Locke manuscripts and in particular about the Essays on the Law of Nature. Von Leyden tells us what new information is available about Locke's early life, and he summarizes two youthful treatises of political theory which take a surprisingly authoritarian position. In dealing with the sources of Locke's Essays on Natural Law, von Leyden gives special emphasis to the influence of Nathaniel Culverwel, a Cambridge Platonist. He also discusses the effect of the Essays on Locke's later work and on subsequent writers. The second part of the volume is taken up by the Latin text and English translation of the Essays. Thirdly, Leyden has included some shorter pieces from the Lovelace Collection, namely, Locke's valedictory speech in laying down the office of Censor of Moral Philosophy at Christ Church in 1664 (Latin text with English translation), and transcripts of a few philosophical notes written by Locke in shorthand.

The qualities of scholarship which von Leyden has brought to his Introduction, his collation of the manuscripts, his notes, and his detective work on Locke's shorthand, are all that could be desired. His English translation of the Natural Law Essays seems to me open to criticism on various points of detail, but no translation of this Latin text could satisfy all readers.

The Essays on the Law of Nature are virtually a treatise on the foundations of ethics, and despite Locke's contention that our knowledge of right and wrong begins with sense-experience the position taken up is a firm version of Ethical Rationalism. In the *Essay concerning Human Understanding* Locke's sketchy remarks about ethics suggest that he held two theories which cannot easily be reconciled, a rationalistic theory of moral rules and a naturalistic theory of good and evil. Von Leyden suggests that the second view grew more prominent in Locke's mind as time went on and that this may explain his failure to publish the Essays on the Law of Nature.

A more general point about Locke's development arises from Essay IV, where Locke argues that 'the light of nature', the natural capacity of getting to know moral truths, consists of reason and sense-perception, and that the first of these depends on the second. Here, plainly (as von Leyden shows in detail), was the original impetus to his epistemological investigations. Like so many other philosophers, Locke was set on the paths of metaphysics (or epistemology) by problems of ethics.

Essays IV and VI are the most interesting philosophically. In Essay IV Locke argues that we may reach a knowledge of absolute moral truth in the following way. By sense-perception we learn of the existence of the world and its regular order. From this, and from the existence of man, we may infer the existence of a creator more *powerful* and *wise* than ourselves. He proceeds from this proposition to the moral judgment that we ought to obey God by glorifying him. Locke seems to think we can infer from God's power *that* we are obliged, and from his wisdom *what* we are obliged to do. He apparently assumes that power entails authority and a right to obedience. He then argues that a wise creator must act for a purpose, and since the creation is 'the work of a most

perfect and wise maker' his aim must be his own glory. It is not made clear just how the last conclusion follows, and in any case its premiss has covertly introduced the additional divine attribute of perfection.

In Essay VI Locke returns to the ground of obligation. He begins with a clear distinction between moral obligation and the prudential motive of being constrained by fear of punishment. The first depends on God's authority, not his power. Yet Locke finds himself unable to maintain a thorough distinction between these two notions. At the bottom of page 182, *potestas* is contrasted with *potentia*, as 'authority' with 'power'. Elsewhere, however, Locke uses the word *potestas* to mean 'power', contrasting it with *ius*. The same goes for the word *dominium*, which is used to mean now 'authority' and now 'power'. Again, he says we are 'subject' to God *summo iure et summa necessitate*, which suggests a failure to separate moral from factual subjection.

Should we just conclude that Locke, in both essays, has confused the language of morals with the language of description (if indeed it is description) of power and its effects, and leave it at that? Or should we not also take note that the Latin terms he uses are susceptible of the double meaning? If Roman legal and moral thought could develop words for authority (notably the word *potestas*) from words relating to power, ought we to be confident that there *must* be, in *any* language, a logical barrier between the two notions? A persistent 'confusion' in philosophy is not so much a compounding of notions which every non-philosopher keeps separate. It is more a perplexed focusing of attention on the *growth* of a distinction that is still not absolute. And a philosopher who brands this as logical confusion is helping to *build up* a logical barrier; he is not just describing the existing height of the barrier in the language and thought of everyman everywhere.

D. D. RAPHAEL

Aquinas. By F. C. COPLESTON. Penguin Books Ltd., 1955. Pp. 263.

THE appearance of a volume on Aquinas in the admirable Pelican Philosophy Series is welcome. Fr. Copleston brings to his task his accustomed careful scholarship and a conscientious determination to make Aquinas respectable in modern eyes. Many misconceptions are removed, and much information given on Aquinas' approach to the major philosophical topics. Particular attention is paid to some of the more obvious objections to the metaphysical approach; the independence of medieval metaphysics on the one hand of medieval science and on the other hand of theology; the empiricist basis of Thomism; the medieval awareness of linguistic problems and of the difficulties inherent in talk about God—all these are rightly stressed. But an impression remains that the sort of reader for whom the book seems to be primarily intended—the reader who is aware of modern problems and answers and is interested to see whether Thomism has a contribution to make—will find it difficult to see what are the crucial points where Thomism differs from more recent views.

I may mention one or two passages which seemed to me particularly unhelpful in this respect. The doctrine of the immediacy and immanence of knowledge, so central to Thomist epistemology, is hardly brought out

at all; and in days when a distinguished Oxford philosopher can speak of the sensible species as the medieval equivalent of the sense-datum, the tendency to a representationalist interpretation needs specific criticism. The only passage which might be taken as a disclaimer of representationalism is the following (p. 177): "to speak of abstraction is not for Aquinas to cut off the life of the intellect from that of the senses and to say that the mind knows only its own ideas. The universal concept is primarily the modification of the intellect by which a thing is known according to its form or essence." Is this intended as a denial of perceptual representationalism, or conceptualism, or both—or indeed neither? As elsewhere, the expression of what Aquinas denies is too vague and the expression of what he asserts too unfamiliar to make for easy understanding.

Again, we are told that St. Thomas would have had little sympathy with attempts to justify knowledge "from the outside, as it were". But what would this enterprise be like, and who is meant to have attempted it? If Aquinas took "steps to justify our spontaneous conviction that knowledge of truth is attainable by analysing the grounds of this conviction and by drawing attention to indubitable acts of knowing", did not Descartes do just this, or Kant? Their error lay rather in the *type* of knowledge which they regarded as indubitable or as in need of justification.

The discussion of matter and form is perhaps one of the most misleading. The difficult notion of metaphysical analysis, though verbally distinguished from scientific analysis, is not made clear, and the student might be forgiven for getting the impression that matter and form, though not occult entities in one sense, are none the less occult entities whose presence is manifested in phenomena much as the presence of burglars is manifested by noises in the night; and when he reads the author's refusal to hazard an opinion as to whether St. Thomas would have regarded the question of matter and form as solved by modern scientific research, his bewilderment may well be complete.

In general the book suffers from a maldistribution of emphasis, and it leaves us with a piecemeal impression of St. Thomas' views. It is perhaps surprising that certain crucial concepts such as that of the transcendentals and of the analogy of being, which do much to unify the metaphysical scheme, are not mentioned as such; the hierarchy of being receives brief mention, and it is perhaps significant that the angels do not appear in it, but only in a footnote as illustrations of immaterial species. For Copleston is too wary of talking theology. This perhaps is why the only historical background given is a brief account of medieval Aristotelianism; and why stress is laid on the rigour with which St. Thomas distinguished philosophy from theology—as though his main purpose were to conquer by division—to the exclusion of a far more significant fact: that besides allocating a distinct role to the unaided reason Aquinas showed, by his existential metaphysic, that the God of reason is no mere dry abstraction, the God of faith no mere object of irrational devotion; but that the two are one—He Who Is, the Self-existent Being, goal of metaphysician and saint alike. Of this tremendous synthesis we are hardly made aware.

Perhaps the lesson of this book may be adapted from Shelley. It may be necessary these days to defend Aquinas, and to present him as a respectable candidate for membership of the Aristotelian Society; but the effect of the author's work is diminished in exact proportion to the degree in which he compels us to advert to this purpose. And we are compelled only too often.

N. J. Brown

On the Knowledge of Good and Evil. By PHILIP BLAIR RICE. New York: Random House, 1955. Pp. 292. $4.50.

PROFESSOR RICE'S main problem with regard to *goodness* might be expressed in this way: "good" has prescriptive force—it indicates a choice or preference; but unquestionably, it is always the kind of choice which we try to support with reasons. These reasons are not mere persuasive tricks: they connect with our choice. What sort of reasons do we offer? That the thing chosen has certain natural properties: it is *because* it has these properties that the thing is judged to be good. What can this amount to, but that there is some logical connexion, some connexion of meaning, between saying that x is good and saying that it has certain natural properites (which make it good)? But is this not to commit the Naturalistic Fallacy? Mr. Rice wants to offer a theory of value-judgements which avoids this "fallacy", but one which is naturalistic in the wider sense—makes no appeal to intuition or to the transcendent.

The book opens with a discussion of four contemporary schools of moralists. The Empiricists base their account of good on desire (R. B. Perry) or on satisfaction (C. I. Lewis) and do not seem to escape the charge of limiting by definition the kinds of thing that can be good. The Intuitionists appeal to an out-dated notion of self-evidence, and they tend to put a stop to the discussion of what is good or right. "If we do not share Ross's intuitions we are mentally and morally immature"—which suggests "the time-honoured upper-class British doctrine of 'good form' or 'doing the right thing'. A gentleman will know what these mean; a bounder will not, and with a bounder there is no arguing" (p. 55). Mr. Rice also objects that we have a right to be suspicious of a doctrine which invokes "non-natural properties" whose genesis is "unintelligible"—which "cannot be conceived as having originated at all, except by an *ad hoc* miracle" (p. 183). The Emotivists (C. L. Stevenson in particular) do not give a satisfactory account of ethical argument. The fourth school discussed is called "the Oxford Moralists" and includes Austin, Hart, Hare, Toulmin, Hampshire and Margaret Macdonald. (Professor John Wisdom is not mentioned: Nowell-Smith's book was evidently not yet at hand.) British philosophers (according to Mr. Rice) acquire their opinions about American philosophy by divination. Mr. Rice has himself followed the more arduous and reliable method of reading the British journals and books. He regards the Oxford Moralists as having shown conclusively that "good" and "right" are not simply descriptive terms. He thinks, too, that they may have gone as far as can be gone in showing that moral verdicts are argued and reasoned. What has now to be shown, is *how* the reasons are connected with the verdict. It is not enough to say that we "just see" or "just know" that a given reason supports a conclusion. "The view that a reason can be relevant to the acceptance of a conclusion without being related to it at all is totally unintelligible to me" (p. 148). Mr. Hare's account of meaning and criteria will not do: he combines the prescriptive with the descriptive but it is only a shot-gun wedding (p. 85). The author then tries to clarify the connexion by reconsidering the larger context in which evaluations are offered (ch. 6-11).

What Mr. Rice really wants (I suspect) is (1) to find something common to the criteria which we apply in judging things to be good of different kinds. A knife is good if it is sharp: a foil is good if it is blunt. But must there not be some property which underlies this apparent difference?

Something which is "conferred" by sharpness in the class of knives and by non-sharpness in the class of foils, but which has the same identity in all classes. And he wants (2) to find something common to the value-judgements of different people: must not all our basic value-judgements show some agreement—rest upon "the deepest affinities of the human creature"? If we could find some property or properties by reference to which we all pick on good things of all kinds, then (in the author's view) it would be part of what "good" means. Not the whole: for in saying that a knife is good we are judging it, choosing it, prescribing it for others and this part of the meaning of "good" (here called the matrix-meaning) is not descriptive at all.

Mr. Rice succeeds in saying something on the whole sound and useful about the meaning of "good". What is good has (in his terminology) certain *constitutive properties*. But what these amount to, is simply that we use "good" in contexts of choosing with reasons, choosing means to ends, choosing on general grounds, and so on. Unless the move we are making were of this kind, we should not use a pure evaluative word like "good". But Mr. Rice wants to go further. He wants to find the *identifying property* which *at bottom* guides our choices. And he fixes (very naturally) on the property of "being enjoyed" or "being enjoyed in the long run". That good things are pleasant is "the presupposition of the action of most men most of the time" and the one they are, on the whole, prepared to stick to, when it is challenged (p. 146).

This (in a much simplified form) is Mr. Rice's account of "good". I shall have to pass over the equally interesting but less fundamental account of "ought" (ch. 13-16).

It will be seen that the author in effect abandons the discussion of "good of a certain kind" and goes back to a search for intrinsic goodness. He therefore has to accept as fundamental, the philosopher's distinction between intrinsic and instrumental goods.

Having chosen "enjoyable in the long run" Mr. Rice recognizes that other choices are possible. (Are they? Only if we can regard "enjoyable in the long run" as a non-evaluative description of a certain *kind* of experience: which is evidently Mr. Rice's view of it.) We cannot, then, show that pleasure is the only possible human preference: and even if we could, human nature might change. Can the philosopher be justified in "adopting" a particular definition of the cognitive sense of 'good'?

I gather that Mr. Rice thinks a philosopher may be so justified. Sometimes he seems to compare this move with a scientist's choice of a technical term. Here of course there may be some choice: and one might try to justify a given choice by showing that it introduced simplicity or order into a technical discussion. A moral philosopher might wish to do this—as, for example, to speak of "prescriptive meaning" in contrast to "descriptive meaning". But the move Mr. Rice wants to make has nothing to do with technical terms: it has to do with the meaning of the word "good". To say that *x is good* is to commend it: and we ask: "Is it also to imply that *x* has some property, *e.g.* that of being enjoyable in the long run?" This is not a question we can answer according to our choice. If there is no such implication then *x is good* does not mean *x pleases in the long run*, and that is the end of the matter.

Does Mr. Rice merely want to insist that only things which please in the long run are *generally* judged to be good? Does he want to offer the opinion that such things *are* good? What he really wants is *to prove it*. He wants to show that *x is good* not only expresses a valuation of *x*: it

also ascribes to x the basic natural property for the sake of which things are valued. Of course many value-words do indeed ascribe properties: "His action was courageous" implies that he faced danger. But so does "His action was cowardly". We judge an action done in face of danger to be courageous or cowardly by reference to certain properties which our standards of conduct in such circumstances lead us to look for: but are these really part of what "courageous" or "cowardly" describes? What Mr. Rice wants is that the fundamental property in virtue of which we call things *good*, should be part of the descriptive meaning of "good".

However, he wants this with so many sensible and shrewd acknowledgments of the impossibility of *having* it, that his long and difficult exploration is of value to the reader. And, after all, his question is a difficult one: Can anything be said *in general* about the connexion between the criteria of goodness and the meaning of "good"? And if so, does it tend to undermine the distinction between the two?

NOTE: Since writing my review I have learned with great regret that the author of this book was killed in an automobile accident on 28 January.

KARL BRITTON

Plato's Philosophy of Mathematics. By ANDERS WEDBERG. Almqvist and Wiksall, Stockholm, 1955. Pp. 154. 19 Sw. Crowns.

THIS book is lucid, wieldy, and undogmatic—three rare virtues in Platonic studies. Professor Wedberg's central problem is that concerning the 'Intermediates': Is Aristotle correct in attributing to Plato belief in objects of pure mathematical study distinct from both mathematical Forms and sensible particulars, or do these Forms exhaust, for Plato, the domain studied by pure mathematics? Wedberg argues tentatively for the former view. There is an illuminating chapter on the theory of Forms in general (ch. iii); chapters iv and v offer a coherent and reasoned account of a philosophy of geometry and of arithmetic respectively, in a series of propositions which the author is prepared to attribute to Plato; finally, there are four appendices, quoting (in English translation) and interpreting the most familiar Platonic and Aristotelian texts that bear on the problem.

Wedberg tackles his problem with a special method of interpretation, introduced and defended in chapter i, which he calls that of 'projection': statements by Plato and Aristotle, of whatever probable date, are 'projected on a plane of simultaneity' and then compared; where Aristotle's statements are consistent with Plato's (*e.g.* are 'near consequences' of them), they are both collected together into a 'system of propositions' which Wedberg regards as Plato's philosophy of mathematics; where Aristotle's statements contradict Plato's, Wedberg suggests that the former are 'conclusions that Plato actually (sometime, somehow) may have drawn'. Though the validity of this method, when applied to Plato on mathematics, is defended at length, in the reviewer's opinion it is a weakness in Wedberg's account. It seems to mislead him on several occasions. For example, though its scope is restricted in chapter i to Plato on mathematics, the method is in fact used in chapter iii on the theory of Forms; seen thus 'projected', the theory indeed embodies a contradiction, that a Form both does and does not participate in itself, which Wedberg calls the 'fundamental antinomy' of the theory (p. 36). But one proposition

used to obtain this contradiction, that the form of Y-ness is itself (a) Y, is essentially part of the early *παράδειγμα* theory, and may well have been abandoned by the time Plato wrote the *Sophist*, where Forms, now usually '*γένη*', are very like *classes*; thus the antinomy probably arises from conflating two Platonic theories of different date. However, it is a pleasure to find *some* method being applied in Platonic research.

As to geometrical Intermediates, Wedberg admits that there is no Platonic passage where their existence is unambiguously asserted: he rightly refuses to use the notorious '*αὐτὰ τὰ ἴσα*' (*Phaedo* 74c1) as evidence, on the grounds that the Greek plural is a perfectly natural idiom for a *relation*-concept like 'equality' (*cf. Parmenides* 129b1-2) and so probably designates a Form (pp. 98-99). The author grants himself too much, however, from the *Republic*'s Line: the 'logic' of the Line may implicitly require a class of special mathematical entities (p. 106), though Ross has denied this (PTI, pp. 62-65), but Wedberg quotes (p. 103), and then forgets (p. 106), 510d7-8, where the mathematician is explicitly said to be concerned with 'the square in itself and the diagonal in itself', certainly Forms. Arithmetical Intermediates are better documented, in Wedberg's opinion (Appendix D). But Wedberg seems to assume that, wherever in Plato numbers are spoken of as sets of units, number-Forms cannot be meant, since for Plato *all* Forms were simple and uncompounded (p. 66). Aristotle, however, frequently says that Plato believed in number-Forms that were sets of units, as Wedberg notes on page 35; Plato himself sometimes speaks as though Forms were like classes; and it may even have seemed *obvious* to Plato that the two itself was two and not one. Finally, the passage *Phaedo* 101b-d need not refer to Intermediates; it seems more natural to suppose Plato here to be talking about one (two) sensible particular(s) rather than the mathematical numbers one and two (*cf.* 101c6, '*τὰ μέλλοντα δύο ἔσεσθαι*'). Thus the dialogues seem as inconclusive about arithmetical as about geometrical Intermediates. There remains the historical question as to how to interpret Aristotle's explicit statements on the matter in view of this fact, but Wedberg hardly touches this problem.

Apart from documentary evidence, Wedberg has one powerful philosophical argument in his favour (pp. 53-56). Plato certainly believed that (1) geometry is true, (2) there are no perfect instances of geometrical Forms in the sensible world, and (3) the universe is divided into the sensible and the intelligible worlds. *If* Plato also believed that (4) the truth of geometry presupposes the existence of perfect instances of geometrical Forms, it is likely that he drew the consequence that such instances exist in the intelligible world. Wedberg gives four arguments for Plato's believing (4), and repeats them *mutatis mutandis* for arithmetic (p. 67). Separately inconclusive, collectively they carry some weight; and we might add a fifth very obvious argument, not mentioned by Wedberg: theorems about *two* circles and statements like '$3 + 3 = 6$' *cannot* merely be about the single forms Circle and Three, and Plato may have seen this.

There is a great deal of real value in Wedberg's detailed analyses both of texts and of arguments. The problem about Intermediates is not solved, but very considerably clarified, by this book. There are some misprints; these may mislead—page 137, l. 21, for 'III' read 'II'; page 138, l. 14, for 'IV' read 'III'; and l. 18, for 'III and IV' read 'II and III'. An index would have been helpful.

E. J. LEMMON

La Révélation d'Hermès Trismégiste. By A.-J. FESTUGIÈRE. Paris: Librairie Lecoffre. Vol. III: *Les Doctrines de L'Âme*, 1953. Pp. xiv + 314. Vol. IV: *Le Dieu Inconnu et La Gnose*, 1954. Pp. xi + 315.

THESE two volumes complete Father Festugière's magisterial study of the Hermetic writings and of Hermetism in many aspects, philosophical, theological and mystical. One of his main themes is the extent to which the pagan Gnosis is connected with the main Platonic tradition. It will be remembered that vol. ii (*Le Dieu Cosmique*) contained important discussions of Plato (specially the *Timaeus*), of Aristotle (specially the *De Philosophia*), and of Stoicism and Eclecticism. Vol. iii starts by making a comparison of Tertullian's *De Anima* with relevant parts of Aetius, Albinus and Iamblichus, so as to establish what the regular second century treatises on the soul were like, and that they derived from the Platonic tradition. Then analysis of the *Poimandres* and other Gnostic writings shows their indebtedness to that Platonic ancestry, as well as illuminating the important differences in form and spirit. Appendices to this volume contain a translation (with full notes) of Iamblichus' *περὶ ψυχῆς*, and a translation of Porphyry's essay on how the embryo receives its soul.

Vol. iv falls into two parts, the first on the transcendent god, the second on mystical knowledge of god. The first part again divides into two, a study of the transcendence of the One and a study of the transcendence of the ineffable god. Academic and Pythagorean speculations are the origin of the former idea, while Festugière appeals to Plato himself (*e.g. Symposium, Republic, Parmenides*) as the first source of the notion of the transcendent god. Texts showing what was taught as Platonic doctrine on the transcendent god are set beside the Hermetic texts on the same subject: 'j'ai essayé de montrer . . . que la notion du *θεὸς ἄγνωστος*, du moins dans la gnose païenne, ne vient pas de l'Orient, mais qu'elle résulte de traditions platoniciennes et pythagoriciennes qu'on peut suivre depuis l'Ancienne Académie'. The second part of the volume studies, mainly in Hermetic writings, ideas about mystical knowledge of god, divided into 'la mystique par extraversion' (man loses himself in god) and 'la mystique par introversion' (man is regenerated by god entering into him). Appendices contain a translation (with notes) of Proclus' commentary on Plato's *Timaeus* 28 c 3 ('to find god is difficult, to tell of him impossible'); and of the passage in his commentary (I. 303 ff.) where Proclus rehearses the views of previous writers on the nature of the demiurge.

It would be impertinent to praise the erudition and insight displayed in this great work; and impossible to attempt discussion here either of its main themes or of particular arguments and interpretations. One small point may be worth mention. Here (iv. 80), as in his book *Contemplation et Vie Contemplative selon Platon* (pp. 229 ff.), Festugière understands Plato to say in the *Symposium* (211 a 7) that of the first principle there can be neither definition nor knowledge (*οὐδέ τις λόγος οὐδέ τις ἐπιστήμη*). The same interpretation has been used, for a very different purpose, by Professor R. C. Cross in his paper on Logos and Forms in Plato (MIND, 1954, p. 442). In fact, however, the text does not say that there is no logos or knowledge *of* Beauty, but that the Form Beauty when 'seen' by itself is not seen either as a face (or other bodily thing) or as a logos or *ἐπιστήμη*: the Form is to be distinguished from all exemplifications of it, whether physical or not. Both *λόγοι* and *ἐπιστήμαι* are mentioned in

the account of the gradual ascent to the Form ; but the Form itself is no more to be *identified with* any particular logos or ἐπιστήμη (οὐδέ τις λόγος οὐδέ τις ἐπιστήμη) than it is with any particular beautiful face (πρόσωπόν τι).

J. L. ACKRILL

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Philosophical Studies, No. 1, Bulletin of the Faculty of Literature, Kyushu University, Japan, 1956, pp. 105.

X.—NOTES

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An annual prize of $100 is offered by the Department of Philosophy, Princeton University, in accordance with the terms of a bequest from J. Walker Tomb of South Africa for a philosophical essay written in the English language on Time, with some reference to Mr. Tomb's own essay on this subject. (Copies may be secured free of charge on application to the Department.) Two classes of entries will be considered: I. From Princeton undergraduates, submitting an essay conforming to the specifications of a senior thesis. II. From members (or associate members) of the American Philosophical Association, or of the Mind Association, submitting an original essay on this topic, published or unpublished, of not less than 5,000 words (if unpublished the essay must be typewritten). In Class II entries should be made by May 1. The department reserves the right to make no award in any year in which, in its own judgement, no entry of sufficient merit is received.

ERRATUM

In N. Brown's article "Sense Data and Material Objects" of MIND April 1957, p. 176, line 20 should read "Assuming f to be a simple sensible quality, f is a secondary quality by definition."

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